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BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers in the present Volume, are Sir H. C. Englefield, Bart. M. P. F. R. S.; Mr. H. Steinhauer; Mr. John Gough; Ra. Thicknesse, Esq.; Mr. Wm. Wilson; C. Wilkinson, Esq.; J. Whitley Boswell, Esq.; Mr. W. Brandé; Aletes; Mr. John Dalton; Capt. J. Mortlock; W. F. S.; Mr. Charles Sylvester; Mr. Ezekiel Walker; T. B. C.; Jos. Huddart, Esq. F. R. S.; E. O.; W. B.; Mr. N. Mendelſhon; C. L.; Mr. Accum; Right Hon. Sir Joseph Banks, Bart. P. R. S. &c. &c.; Mr. R. Matthews; Mr. J. Biddle; An Old Correspondent.—Of Foreign Works, Thenard; Hesinger; Bergelius; Morozzo; Bucholz; Dartigues; Schoerbing; Boullay; Lagrange; Lussac; Erman.—And of English Memoirs, abridged or extracted; Smithson Tennant, Esq. F. R. S.; W. H. Wollaston, M. D. F. R. S.; C. Hatchett, Esq. F. R. S.; Gregory Watt, Esq.; Right Hon. C. Greville, V. P. R. S.; George Pearson, M. D. F. R. S.; J. Hinckley, Esq.; F. S. A.; T. A. Knight, Esq.

Of the Engravings the Subjects are, 1. An ancient Egyptian Engraving in Symbols, supposed to be astronomical. 2. Figures to illustrate the Galvanic Charge, by C. Wilkinson, Esq. 3. Outlines of Pendulous Measures of Time, by J. W. Boswell, Esq. 4. Figure to illustrate Mr. Gough's Theory of the Augmentation of Sound, by communicated Vibrations. 5. Instrument for surveying, by Capt. J. Mortlock. 6. Instrument for taking Designs from Nature. 7. Plan and Sections of an improved Air-Pump, by Mr. Mendelsshon. 8. Figures to illustrate Mr. Gough's Theory of the

the Speaking Trumpet. 9. Improvement in Woulfe's Apparatus. 10. Method of drawing a Line in a Circle equal to the Side of its Square. 11. Two quarto Plates, exhibiting highly magnified Representations of the Parasitic Plant which causes the Blight in Corn, from Drawings by M Bauer, Botanical Painter to the King. 12. Mathematical Diagrams of the Properties of Triangles, by Mr. Gough. 13. Application of Spectacle Glasses, by Mr. E. Walker.

Soho Square, April, 1805.

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MARCH.

MARCH, 1805.

APRIL,

APRIL, 1805.

A

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JANUARY, 1805.

ARTICLE I.

Concerning the original Inventors of certain philosophical Discoveries: the Reflection of Cold; Compression of Water in a metallic Vessel; the Telescope; and a perspective Instrument formerly described. In a Letter from Sir H. C. ENGLEFIELD, *Bart. M. P. F. R. S. &c. &c.*

To Mr. NICHOLSON.

SIR,

ALTHOUGH abstractedly speaking, it is of no importance to science who first made the most valuable experiments, or to what individuals the world is indebted for the most useful discoveries; yet as the sense of mankind has universally in this, as in several other things, been in direct opposition to frigid reasoning, and every one has been desirous to vindicate the fame of eminent men, by ascertaining to them the honour due to their labour or genius, I trust that the following observations will not be deemed unworthy a place in your valuable Journal.

Universal sentiment in favour of discoverers.

1. The experiment of the reflexion of cold has been, as far as I know, both in conversation and in printed books, uniformly ascribed to Mr. Pictet of Geneva. Now in the collection

Reflection of cold ascribed to Pictet.

It was before made by the Acad. del Cimento.

tion of experiments publiſhed by the Academy Del Cimento, of Florence, in the year 1666, the identical experiment is deſcribed in the following words:

" *Ninth Experiment.*

Narrative.

" We were deſirous to try whether a concave ſpeculum, expoſed to a maſs of ice weighing 500 pounds, would reflect any ſenſible degree of cold on a very delicate thermometer of 400 degrees, placed in it focus. The reſult was, that the thermometer inſtantly ſunk: but a doubt remained whether the thermometer was acted on more by the direct cold of the ice, or that reflected by the ſpeculum. This doubt was removed by covering the ſpeculum: and certain it is (whatſoever might be the cauſe) that the ſpirit inſtantly began to riſe again. Yet ſtill we will not preſume poſitively to affirm, that this riſe might not have been owing to ſome other cauſe than the taking off the reflection from the ſpeculum, all the precautions not having been taken which might be conſidered neceſſary to ſecure abſolute aſſent to the experiment."—*Saggi di Naturali Eſperienze, page* 176.

I do not mean to aſſert that Mr. Pictet had ſeen this moſt excellent book, or that he borrowed from it without acknowledging his obligation; but the honour of having firſt made the experiment is certainly due to the Florentine philoſophers.

Excellency of their publication.

It may not be foreign from the ſubject here to obſerve, that the "Saggi," for perſpicuity, brevity, and that diffident caution ſo eſſential to the inveſtigation of truth, is a model of philoſophical writing, not perhaps excelled by any book written ſince that time, and more admirable when we conſider the diffuſe and obſcure ſtile ſo much in faſhion in the works of the learned at that period.

The Acad. del Cimento ſaid, in moſt books, to have compreſſed water in a globe of gold.

2. Scarcely any treatiſe on natural philoſophy has failed to quote an experiment made by the academicians Del Cimento, on the incompreſſibility of water incloſed in a globe of gold, and ſubmitted to the preſſure of a ſcrew preſs. Mr. Canton, in his experiments on the ſame ſubject, publiſhed in the 52d vol. of the Phil. Tranſ. ſo ſpeaks of it. You will probably be ſurpriſed when I aſſert, that the academicians did not try the

The globe was ſilver, and compreſſed by hammering.

experiment with a gold globe, but one of ſilver; and that they give a reaſon for uſing ſilver in preference to gold, namely, that the ductility of gold was ſo great that it would extend

extend its dimenſions by preſſure, and ſo defeat the intention of the experiment. Neither was the globe compreſſed by a machine, but merely by hammering.

Lord Bacon firſt made this experiment.

But the honour of this experiment is not due to the Florentine philoſophers, but to our own illuſtrious countryman Lord Bacon, who, in the *Novum Organum*, Book II. Sect. 45, gives the very ſame experiment on water encloſed in a large and ſtrong globe of lead, which being firſt compreſſed by the hammer and then by a ſcrew-preſs, the water exuded through the pores of the metal, and ſtood in a dew on its ſurface.

The inaccuracy with which this experiment has been related, though in this caſe of no great importance, may however be an uſeful leſſon, and induce thoſe who wiſh to attain to truth, to truſt as little as poſſible to information at ſecond-hand, but to recur to the original authority whenever it is practicable to do ſo.

The teleſcope was uſed by Kepler in aſtronomy, two years before Galileo.

Before I conclude, permit me to mention a curious circumſtance relative to the æra of the invention of the teleſcope. Almoſt all books place it in the year 1609, and the firſt diſcoveries of Galileo were made in 1610; yet Kepler, in his account of the comet which appeared in September, 1607, (*See Kepler de Cometis, page* 25) expreſsly ſtates, that though, to the naked eye, it was only equal to certain fixed ſtars near it, yet, when viewed with teleſcopes *(perſpicillis)*, it appeared larger than thoſe ſtars.

Remarks on the progreſs of diſcovery, &c.

Although the intercourſe between the different nations of Europe was at that time much leſs than it now is, it is very difficult to ſuppoſe that Galileo ſhould not, in upwards of two years, have heard, not only of the invention, but its application to celeſtial obſervations by a man ſo eminent in ſcience as Kepler: and it is ſcarcely leſs extraordinary that Kepler himſelf ſhould not, in that ſpace of time, have foreſtalled Galileo in ſome of thoſe diſcoveries which rendered his name ſo illuſtrious; for however imperfect Kepler's teleſcope might be, it is hardly poſſible that it could have failed to ſhew him the ſatellites of Jupiter, which are viſible to the commoneſt opera glaſs.

I am, Sir,

Your obedient ſervant,

H. C. ENGLEFIELD.

Tilney Street, Nov. 30, 1804.

Ramfden was the inventor of a telefcopic perfpective inftrument referred to.

P. S. I beg leave to add, that the machine for drawing in perfpective, defcribed in your Journal for October, page 122, was (I am almoft abfolutely certain) the invention of the late moft eminent Mr. Ramfden, and was made by him full 25 years fince, for the Right Honourable Charles Greville.

II.

Account of an ancient Egyptian Sculpture with Hieroglyphics, fuppofed to relate to Aftronomy. In a Letter from Mr. H. STEINHAUER. *With an Engraving.*

To Mr. NICHOLSON.

SIR, *Fulneck, Oct. 25, 1804.*

Porphyry fculptured in the form of a fcarabæus, and bearing hieroglyphics.

PERHAPS the liberty that I take in fubmitting the annexed draught of an Egyptian antiquity, may be an intrufion upon the plan of your valuable Journal. The Egyptian hieroglyphic infcriptions are involved in fuch impenetrable darknefs, that every trifle contributed to their elucidation cannot but be acceptable, indeed equally acceptable to the antiquarian, the hiftorian, and the aftronomer; fince the greater part have probably a reference to that fcience. Stones fculptured in the fhape of a fcarabæus are, I believe, (for I make no pretenfions to the title of an antiquarian) well known; there are a few in the Britifh Mufeum; but I never yet faw either a facfimile of the infcription, or any attempt at an explanation of the fame. The drawings inclofed are taken from a ftone brought by the Swedifh conful at Aleppo to Stockholm, and there prefented by him to a gentleman, who has favoured me with the loan and permiffion to make drawings or impreffions of it at pleafure. The upper part, of the fize of the fketch, obvioufly reprefents a fcarabæus; probably the S. Sacer, Lin. & Fabr. though the indentures on the head are but flightly marked, and a flight damage between the thorax and elytra render it impoffible to difcover whether a fcutellum be indicated or not. It was perhaps the only beetle they reprefented in fculpture, and the accuracy of a modern nomenclator is confequently unneceffary to determine it. If I recollect right, this

this beetle was made use of as a symbol of the annual rotation of the sun. The under side is flat, or slightly convex, perhaps from wear, and bears eight lines of hieroglyphics, probably referring to the change of the seasons. The drawing, made with the greatest accuracy from an impression in sealing-wax, is consequently inverted.* Should you, sir, or any of your friends, wish for one to prove the exactness of the draught, it is very much at your service. The stone itself is apparently a species of porphyry, the *serpentino verde antico,* of a green mass, with very dark green field-spar, and pale green indistinct stripes. The plainness of the inscription, and the same symbol occurring several times upon it, led me to suspect, that it would be no very difficult task, for such as are versant in antiquities of the kind, to discover somewhat of its purport; and being conscious of its value, I was unwilling to let it lie by me, without offering it to the attention of such as are more deeply interested in it. Your wish to promote the arts and sciences, which so obviously pervades your esteemed Repository, makes me hope that you will excuse mine, in case the subject should not be admissible into your Publication.

Porphyry sculptured in the form of a scarabæus, and bearing hieroglyphics.

I remain, Sir,

Your obedient humble servant,

H. STEINHAUER.

P. S. Is the carbonate of barytes a frequent British mineral, except at Chorly? If not, it may perhaps be interesting information, that it is found in very large quantities near Murton in Cumberland, and some other places of the vicinity. The native oxide of zinc, scarcely inferior to the flowers of zinc of the shops, I do not recollect to have seen mentioned. It is found in one part of the West Riding of Yorkshire, in considerable quantities.

Native carbonate of barytes, and oxide of zinc.

* The engraver having followed the drawing upon the copper, our print is not inverted.—N.

Extract

III.

Extract of a Memoir on the fuming Liquor of CADET. *By Cit.* THENARD.*

History of the diſcovery of Cadet's fuming liquor, &c.

CADET diſcovered this liquor near half a century ago, in making inveſtigations on arſenic. The name of its author was given to it then, which it has retained ever ſince, becauſe its intimate nature and conſtituent principles were unknown. The thick ſmoke which this ſingular product ſpreads through the air, its ſpecific gravity, greater than that of water, its oily ſtate, its great volatility, its powerful odour, its ſpontaneous inflammation in the air, obſerved by Cadet and the chemiſts of Dijon; in fine, all its properties, each more extraordinary than the other, induced Cit. Thenard to ſubject it to analyſis.

Method of obtaining it. Diſtillation of acetite of potaſh and arſenious acid.

He began by procuring ſeveral ounces of this liquor, by diſtilling, according to Cadet's proceſs, equal parts of acetite of potaſh and arſenious acid, the produce of which he received in glaſs globes, cooled with a mixture of ice and marine ſalt. A liquor, very little coloured, and ſmelling ſtrongly of garlic, ſoon paſſed into the receivers; at the ſame time much gas was diſengaged, which ſpread the ſame odour, and the receivers were filled with vapours, ſo heavy that they ſeemed to run like oil. When the operation was terminated, he unluted the apparatus, and broke the retort. The bottom of it was covered with a white, acrid, and alkaline ſubſtance of potaſh, ariſing from the acetite employed; and the neck was lined with cryſtals of arſenic, owing to the reduction of the arſenious acid. The gaſes, the quantity of which was very great, contained arſeniated hidrogen, in addition to the carbonated hidrogen and carbonic acid, given by all vegetable matters which are decompoſed by fire. The liquid product was compoſed of two very diſtinct ſtrata, holding metallic arſenic in ſolution, which was not long before it was depoſited in flakes; the upper one was of a browniſh-yellow and aqueous, the lower one was leſs coloured, and of an oily appearance. He ſeparated them, by pouring them into a tube drawn to a point by the lamp, which allowed him to receive them

Reſidue.

The liquid product conſiſts of two diſtinct fluids.

* From Bulletin des Sciences, Tom. III. p. 202.

in

in different veſſels. The heavieſt, as being the moſt uſeful to be known, was firſt examined. At the commencement he was affected by the thick vapours which it diſperſes through the air, and by its extremely penetrating and horribly fetid odour. Its action on the animal economy is ſo powerful, that he found it impoſſible to devote more than one hour in a day to his enquiries, and he was more than once tempted to abandon them. He was in the ſame ſtate as if he had taken a ſtrong medicine, and experienced ſtupefying effects; againſt which he employed ſulphurated hidrogen diſſolved in water, with ſucceſs.

Examination of the heavieſt.

It powerfully affected the animal economy.

As he had but a ſmall quantity of the liquor at his diſpoſal, and it was of importance that his attempts ſhould not be fruitleſs, he regulated the order of his enquiries in this manner: He firſt determined the cauſe of the odour which it ſpreads through the air; then he ſought for that of the thick vapours which it produces, and afterwards that of its ſpontaneous inflammation; and he made uſe of the determination of theſe three points to diſcover the fourth, and moſt important, the conſtituent principles of the ſubſtance.

Order of the inveſtigation.

The odour could only proceed from the ſubſtance itſelf, or elſe from an elaſtic fluid which it might hold in ſolution, and which the author ſuppoſed to be arſeniated hidrogen. He therefore diſtilled, with great care, a certain quantity of the liquor in a ſmall glaſs retort, to which were adapted a receiver and a tube for collecting the gaſes. He obtained nothing but the air of the veſſels; the liquor was entirely volatilized, and paſſed into the receiver without having undergone any alteration, except that its colour was not quite ſo deep. Hence the odour of the arſenical liquor is owing to its property of being volatilized, and, probably, diſſolved in the air.

Cauſe of the odour.

The liquor was unchanged by diſtillation.

The cauſe of its vapours could only be owing to an abſorption of oxigen, or to an abſorption of the water diſſolved in the air, or to both theſe effects at the ſame time. The air of a flaſk, into which Cit. Thenard had poured a few drops, immediately loſt its tranſparency, and, in a ſhort time, became incapable of ſupporting the combuſtion of a taper. A veſſel of the ſame ſize, filled with carbonic acid, offered the ſame phenomena, but in a leſs diſtinct manner. To guard againſt the contact of the air, he had been careful to ſuſpend a very thin tube, containing the liquor, to the cork of the flaſk,

Its vapour is occaſioned by an abſorption of oxigen and water.

ſo that he could eaſily break it againſt the ſides of the veſſel. The vapours were not perceptible when he uſed carbonic acid perfectly dried; whence he concluded, that the vapours of the arſenical liquor are owing to an abſorption of the oxigen and the water contained in the air jointly, but that the firſt of theſe cauſes ſeems to be more powerful than the ſecond.

Its property of inflaming ſpontaneouſly is owing to metallic arſenic.

From this it ſhould ſeem that the arſenical liquor is poſſeſſed of the property of inflaming alone. It does not however take fire at the approach of a body in combuſtion, when it is very pure; and it is obſerved, that, in all the ſpontaneous inflammations which it experiences, the combuſtion always begins at the black ſpecks which diſturb its tranſparency, and are only metallic arſenic very much divided.

It muſt therefore contain arſenic,

It now remained to determine the nature of the arſenical liquor. Its odour, which reſembles arſeniated hidrogen gas, indicated that it muſt contain arſenic, and that this metal muſt have great influence on the phenomena which it offers: Its combuſtibility, its conſiſtence, and its appearance, indicated an oily matter; and, although it did not change the tincture of turnſole, and no re-agent demonſtrated immediately the exiſtence of acetous acid, this body muſt nevertheleſs be expected in it. To ſucceed in inſulating theſe different ſubſtances, the author tried the alkalies; but experience ſoon convinced him that he muſt have recourſe to other means.

an oily matter, and acetous acid; but this was not indicated by alkalies.

Oxigenated muriatic acid decompoſed it.

He employed the oxigenated muriatic acid with much greater ſucceſs. Some drops of the liquor, poured into this gas, were inſtantly inflamed, and their decompoſition was complete. They were then precipitated in white flocks by lime-water, and in yellow ones by ſulphurated hidrogen; while, on being ſaturated with potaſh and evaporated, they formed a foliated ſalt, ſtrongly attracting the humidity of the air, acrid, ſharp, decompoſable by ſulphuric acid, and diſengaging a ſtrong odour of vinegar. The quantity of arſenic and of acetous acid obtained, being far from correſponding with the quantity of liquor which had been uſed, there muſt therefore have been another body in it which it was requiſite to inſulate, and this was accompliſhed by treating a new portion of the liquor with a quantity of water ſufficient to diſſolve it; then on decompoſing it by ſulphurated hidrogen, it yielded a precipitate, ſlightly yellow, very much divided, formed principally of arſenic and ſulphur, which was a conſiderable time in ſepa-

Formed a foliated ſalt with potaſh.

Its ſolution in water, decompoſed by ſulphurated hidrogen, yielded ſulphur, arſenic, and oil, containing acetous acid.

rating

ruting from an oil, which was afterwards feen fwimming on the furface of the liquor. This contained a great deal of acetous acid. Its decompofition may be further facilitated by expofing it to the air; then it emits thick vapours, it is cryftallized, and becomes flightly humid. It is rendered turbid by lime-water, and yields a yellow precipitate with fulphurated hidrogen.

Compofition of the heavieft liquor.

From thefe different experiments it follows, that this liquor is compofed of oil, acetous acid, and arfenic, nearly in a metallic ftate; and that it muft be confidered as a fpecies of foap, with a bafe of acid and arfenic, or as a fort of oleo-arfenical acetite.

The upper fluid differs from the lower, only by having a larger proportion of acetous acid and water.

This analyfis was very ufeful in that of the upper liquor. In fact, notwithftanding the difference which feems to exift between them, fince the latter refembles water, can combine with it in all proportions, forms only a flight cloud in the atmofphere, has much lefs odour, and does not inflame in any cafe, it is eafy to prove that it only differs from the firft by its greater proportion of acetous acid, and by the water which it contains; for it reddens the tincture of turnfole ftrongly, effervefces with the carbonates, gives rife to acetites, and is flightly precipitated of a yellow colour by fulphurated hidrogen, which feparates a little oil from it. A very fmall quantity of oxigenated muriatic acid fpeedily deftroys the odour, and it is then precipitated white by lime-water, and of a deep yellow by the hidro-fulphurets. Expofure to the air produces in it, by time, the fame changes as take place immediately with oxigenated muriatic acid. Finally, a liquor, exactly fimilar, is formed by diffolving a few drops of the lower liquor in very weak vinegar, and thus fynthefis confirms the refults of analyfis.

Theory of the procefs.

We can now eftablifh a theory, clear of all hypothefis, on the phenomena offered by the diftillation of acetite of potafh and arfenious acid; we fee that one part of the arfenious acid is entirely reduced; that another is only brought near the metallic ftate; that the acetite of potafh is totally decompofed; that almoft all the acetous acid is alfo decompofed; and, that from thefe different decompofitions refult water, carbonated hidrogen, arfeniated hidrogen, carbonic acid, a peculiar oil, oxide of arfenic, arfenic, and potafh; that the potafh forms the white refidue found in the veffels in which the diftillation

Component parts of the fuming liquor.

was

was performed; that the arfenic is fublimed, and adheres to the neck of the retort; that the three different fpecies of gas are mixed, and may be collected in flafks; finally, that the water, the oil, the acetous acid, and the oxide of arfenic, are condenfed in the receiver; that thefe three laft bodies, by combining in certain proportions, form a very volatile compound, heavier than water, and fparingly foluble in it; and that it is for this reafon that it feparates into two very diftinct ftrata; the lower of which muft be confidered as an oleo-arfenical acetite, and the other as a portion of the firft diffolved in water, which folution is promoted by an excefs of acetous acid.

IV.

Account of Cerium, a new Metal found in a mineral Subftance from Baftnas, in Sweden. By W. D'HESINGER *and* J. B. BERGELIUS.

(Concluded from Page 300, Vol. IX.)

With Succinic Acid.

Succinate of cerium.

(P.) AT firft, fuccinic acid renders the faturated folutions of cerium turbid, but afterwards they become clear. Thus, a few drops of fuccinate of ammonia, poured into a faturated nitric or muriatic folution of this metal, occafion a precipitate, which foon difappears. On pouring in a greater quantity of this re-agent, the fuccinate of cerium is immediately depofited. This combination is white; it is not wholly infoluble in water, fince the folution from which it was feparated ftill retains a little, as is manifefted by evaporation, or by alkalies. The falt obtained by digefting free fuccinic acid with the oxide, comports itfelf in the fame manner. The acid diffolves it readily. Expofed to the fire, it burns with a blue flame.

Since the acetate of cerium is not precipitated by the fuccinate of ammonia, this is a certain means of obtaining the cerium freed from iron.

With Gallic Acid.

Gallate of cerium.

(Q.) If cryftallized gallic acid is put into a faturated folution of muriate of ferium, a fmall quantity of a white precipitate is

is formed. The alkalies augment it, and give it a clear chocolate colour. If a larger quantity of alkali is gradually added, the quantity of the precipitate, and the intenſity of its colour are increaſed; in proportion as this addition is made, the precipitate becomes of a reddiſh brown, and at length, by expoſure to the light, gives a turbid, deep green ſolution.

With Pruſſic Acid.

(*R.*) The ſaturated ſolutions of cerium are precipitated white by the pruſſiates. The voluminous precipitate has much reſemblance to ſilver precipitated by muriate of ſoda. An exceſs of acid readily diſſolves this precipitate. **Pruſſiate of cerium.**

Oxide of Cerium with Sulphurated Hidrogen.

(*S.*) At the commencement, the ſaturated ſolutions of cerium are precipitated of a browniſh colour by hidro-ſulphuret of ammonia; but if more of this re-agent is added, the precipitate is of a deep green. The muriate of cerium alone takes a deep green colour, but if an alkali is added to it, an hidro-ſulphurated combination of muriate of cerium, at a *minimum* of oxidation, is formed, which is rapidly precipitated of a bright green colour. **Hydro-ſulphuret of cerium.**

The hidro-ſulphurated cerium, well dried, has a deep green colour, almoſt black. It is eaſily deſtroyed by heat; put into a crucible, moderately hot, it burns with a yellowiſh, phoſphoric flame, only viſible in the dark.

Sulphurated Oxide of Cerium.

(*T.*) The ſulphuret of ammonia gives a fading brown precipitate with the ſolutions of cerium. This ſulphuret, added in exceſs, gives a precipitate of a graſs green, which becomes bright green by deſiccation: it burns with a blue flame and the pure oxide remains. **Sulphurated oxide of cerium.**

The muriate of cerium at a *minimum* is precipitated white by ſulphuret of ammonia.

Oxide of Cerium with Phoſphorus.

(*U.*) A piece of pure phoſphorus was put into a ſolution of muriate of cerium contained in a cloſed veſſel, and kept for ſeveral days on a ſtove. The bottom and ſides of the veſſel were covered with a white precipitate, and the piece of phoſphorus **Phoſphate of cerium.**

phorus was covered with a hard brown cruſt, from which the phoſphorus was ſeparated by heating it in warm water. This cruſt was tenacious, and ſhining in the dark. Heated, it took fire like phoſphorus, and left a reſidue of a ſmall quantity of oxide, which, on being melted with borax, exhibited the characters deſcribed above, except that the globule retained its clear green colour, after being cooled. It appears probable that theſe phenomena aroſe from the phoſphorus containing cerium. But the phoſphorus, kept for a month in the ſame ſolution, ſlowly depoſited a white powder; whence it is uncertain whether the formation of the cruſt, mentioned above, was owing to the cerium. The acid was always in exceſs in the ſolution, and the white powder had all the appearance of phoſphate of cerium.

With the Alkalies.

Action of the alkalies.

(V.) The pure alkalies do not diſſolve cerium, even by fuſion. By this means it is eaſily deprived of manganeſe. Pure ammonia digeſted with the oxide, does not diſſolve it, but renders it yellowiſh.

The carbonated alkalies diſſolve the oxide of cerium in ſmall quantity. The ſolution is yellow, and is precipitated by the acids. They alſo diſſolve it by fuſion in covered veſſels. In the open fire, it oxides too much to enter into ſolution.

Iron or zinc do not precipitate the ſolutions of cerium.

The ſolutions of cerium are not precipitated by iron or zinc.

Action of galvaniſm.

The electric pile of Volta only decompoſes the ſalts, and the yellow oxide adheres to the conductor.

V.

New Experiments on Abſorption by Charcoal, made by Means of a new Machine. By C. L. Morozzo.

(Concluded from P. 264 Vol. IX.)

On Oxigen Gas.

Oxigen gas.

25th. I NEXT proceeded to the examination of the abſorption by charcoal in oxigen gas.

I therefore extracted ſome oxigen gas from red precipitate to be employed in my different experiments. Of this gas,

trie

tried with charcoal in my machine, eight inches and ſix lines were abſorbed: this great abſorption ſurprized me; for in my firſt experiments, in 1783, made in cloſed tubes. I had had but a very ſmall one: I therefore found no difficulty in attributing it to my matraſs' having been melted towards the cloſe of the operation, which made me ſuſpect that my gas had been ſpoiled by the nitrous acid; and I repeated the experiment with great care. Oxigen gas.

26th. I then took oxigen gas which had been obtained from red precipitate with great attention, and had been prepared in the laboratory of Doctor Bonvoiſin; what was my aſtoniſhment at obſerving that, in thirty-ſix hours, the abſorption amounted to twelve inches and eight lines, and that it continued for ſeveral days: in forty-eight hours it was fourteen inches ſix lines; in four days, fifteen inches two lines; in five days, ſixteen inches four lines; and, finally, in three days more, the gas was entirely abſorbed.

27th. This experiment was too intereſting to omit repeating it with the greateſt poſſible preciſion.

I therefore took ſome of the ſame oxigen gas, and the abſorption was twelve inches three lines in the firſt four hours; in forty-eight hours, it was thirteen inches ſix lines; in three days, fourteen inches five lines; at the end of the fourth day, it was fifteen inches three lines? of the fifth, ſixteen inches; of the ſixth, ſixteen inches ſeven lines; of the ſeventh, ſeventeen inches two lines; finally, on the eighth day the gas was entirely abſorbed.

28th. I repeated the ſame experiment on oxigen gas obtained from water expoſed to the ſun, in which I had put three ounces of charcoal-powder *; in this gas, which was very pure, the abſorptions took place in the ſame manner.

29th. Theſe facts will ſurpriſe thoſe who, having made their experiments on oxigen gas in tubes cloſed hermetically, and by paſſing the charcoal through the mercury, obtained only very ſmall abſorptions, which never exceeded three inches

* In the twelfth volume of the Italian Society, I have inſerted a memoir on the property which charcoal mixed with water has of developing one-third more of oxigen gas, of a ſuperior quality to that obtained from pure water: In this memoir I prove, that a part of this oxigen gas is produced by the charcoal, which communicates the *principle of fire* to the air contained in the water.

three

Oxigen gas. three lines in tubes of twelve inches in height. I am certainly not miftaken; and M M. Rouppe and Van-Norder, who have repeated my experiments in their apparatufes, have obtained the fame refults, and have found, as I did, that, next to hidrogen, oxigen gas is that which is leaft abforbed by charcoal.*

30th. I muft not pafs over a fingular experiment, the explanation of which will alfo be very difficult.

I examined the abforption which a piece of charcoal, that had remained feven hours in the bright light of the fun, had effected in the fame oxigen gas; it was only between feven and eight lines, while a fimilar piece, which had been only five hours in the folar light, produced an abforption of ten inches three lines in carbonic acid gas.

It muft be obferved, that a piece of charcoal expofed to the light of the fun and placed in oxigen gas, produced only an abforption of feven or eight lines, juft like the abforption of a piece which had been expofed to the fun's light, and placed in hidrogen gas, which gave only an abforption of fix lines: and, in this, thefe refults are perfectly conformable to my experiments, made in the year 1783.

31ft. This difference of the refults is therefore only owing to the method either of paffing the charcoal under the mercury, or of leaving it in the machine. It appears that, by paffing it through the mercury, the charcoal lofes much of its attractive power, which it retains in the machine.

Let us endeavour to find whether thefe differences can be accounted for.

32d. It might be fuppofed that the red-hot charcoal inflames the oxigen gas; but fince I do not open the key until fome time after the charcoal has been inferted, it does not appear to me to be likely that the gas can be inflamed: befides, if this were the cafe, the abforption would be made at once, and it would not require eight days to be complete.

33d. It might be fuppofed that, in this cafe, the charcoal fupplied hidrogen gas, which, mixing with the oxigen gas and producing water, would caufe the abforption: but during the whole experiment there was not the fmalleft drop of water perceived in the tube.

* Ann. de Chimie.

34th. There might be reafon to believe that the charcoal, by inflaming the oxigen gas, might convert it into carbonic acid gas, which is the moft abforbed of all the gafes; but to fatisfy myfelf of this, I produced an abforption of oxigen gas by a piece of charcoal, as in the former experiments: in twenty-three hours it amounted to twelve inches, then with a fyringe of cryftal I paffed fome lime-water through the mercury into the remaining gas, and it did not become turbid, which proves that carbonic acid was not formed; and, having turned up the tube, the refidual gas extinguifhed a taper: I believe it to have been azote.

35th. I was of opinion that incandefcent charcoal would abforb more than another piece which had been fuffered to cool till it no longer appeared red. Experiment fhews a greater abforption in the firft for twenty-four hours; but, leaving the apparatus undifturbed, in two days the abforption became equal. I operated upon atmofpheric air.

36th. I muft acknowledge my inability to give an explanation of thefe fingular experiments, as I have already declared above: were I to hazard any conjectures, they would probably be overturned by new theories, which do not fpare even the labours of the greateft chemifts in France.

37th. I fhall therefore continue my operations as I propofed, and new experiments will, at leaft, fupply the want of fyftems.

On the Difference in the Charcoals made ufe of.

38th. Having afcertained the abforptions effected by the charcoal of beech wood in atmofpheric air and in the different gafes, I proceeded to examine the variations which the charcoals of different woods would produce in them. **Difference in the charcoals made ufe of.**

39th. I therefore took two pieces of charcoal, the one of beech, the other of the branches of fallow deprived of bark: I introduced them both in an incandefcent ftate, and examined the difference of their abforption in the fame atmofpheric air. **Charcoal of fallow;**

That of beech gave an abforption of feven inches and eight lines in fix hours.

That of fallow gave only an abforption of four inches and three lines, and there was no further abforption after fix hours.

Thefe

These experiments correspond perfectly with those which I have published in my third memoir. I shall dispense with the relation of several other experiments which I have made on different woods, such as hazle, vine-twigs, &c. all of which gave less absorptions.

of box;

I afterwards tried the charcoal of box wood, although it is a very compact wood; but since Kirwan found that this wood contains much more saline matter than any other wood, it was desirable to submit it to experiment: its absorption, however, was considerable, and equal to that produced by beech-wood. The greater or less quantity of saline matter in the wood of which the charcoal is made, therefore, does not contribute to the variations in the absorption.

of cork.

40th. I afterwards examined the charcoal of cork, which interested me greatly, because Cit. Odier, from the experiments of Dr. Beddoes, has proposed it as a medicine: I was desirous of examining whether it contained more of the principle of fire than the others. I therefore charred cork; but it is so light, that, after the charring, a piece of equal bulk with that of the beech which I had used, weighed only three grains: to bring the circumstances to an equality, it was therefore necessary to make the experiment with a piece of beech charcoal which weighed three grains *: it was in atmospheric air it was tried. The following are the results:

Three grains of beech charcoal produced an absorption of one inch and nine lines.

The charcoal from cork absorbed only nine lines.

The charcoal of cork contains more of the principle of fire than any other charcoal.

41st. According to my principles †, the charcoal of cork must contain the matter of fire more than any other charcoal; thus when it is necessary to give it as a medicine, either inwardly or outwardly, it should be preferred to all the others; and I am of opinion that it is by oxidating, and not by disoxidating, that it produces its astonishing effects. Besides, we have instances of substances which act in this manner. The oxigenated muriatic acid, far from abstracting the oxigen, is

* The pieces of beech charcoal which I use, and which weigh thirty-six grains, are of the length of ten lines, and about five in diameter.

† See my third memoir on charcoal.

supposed

fuppofed, on the contrary, to add a confiderable quantity of it to fetid ulcers, and carries off their fetidity as well as charcoal.*

Water may fometimes be ufed in the Inftrument inftead of Mercury.

42d. Although, in the courfe of thefe experiments, I have conftantly employed mercury, in fome experiments a coloured water may be ufed with advantage, fince it is better feen in the tube; but this cannot be ufed when the gafes which are abforbed by water are examined, nor with the carbonic acid and the other acid gafes which are much abforbed: then it is neceffary to change the tube, and to fubftitute one which, inftead of eighteen inches, is at leaft thirty-fix in height; without which it will be entirely filled.

Coloured water may fometimes be fubftituted to mercury in the machine.

43d. It was on atmofpheric air taken in my room with the windows open, that I experimented with coloured water. The charcoal produced an abforption of ten inches and a half. It has been feen, that, with the mercurial apparatus, I had had an abforption of eight inches fome lines.

The preffure of the atmofphere in the water therefore augmented the abforption, which ufually takes place with the mercury, by two inches and a half; that is to fay, one-fourth of the height.

The preffure of the atmofphere affects the refults.

44th. I fhould obferve, that very frequently the fmall differences of one or two lines obferved in the abforptions of the fame quality of air or of gas, are owing to the variations of the atmofphere, which I afcertained by comparing the height of the barometer during the experiment.

The Pieces of Charcoal ufed in thefe Experiments, acquire Weight.

45th. After the experiments, all the pieces of charcoal employed had acquired greater weight: this augmentation was from half a grain to two grains, and appears to be dependent on the greater abforption.

The charcoal acquires weight in thefe experiments;

46th. I took four pieces of charcoal which had been ufed in the experiments made with atmofpheric air, which, among them, had acquired four grains in weight: I paffed them

* See the notes of M. Odier to Beddoes's memoir. Bibl. Brit. Vol. VI. p. 359.

under water, in a tube of one inch in diameter and fix inches high, which was alfo filled with water. A part of the air which they contained was immediately developed, but I only obtained about two inches, (they had, however, abforbed among them nearly thirty-two inches). The reftored air was found to be compofed of carbonic acid gas and azote gas. In this operation a part of the air is fixed in the charcoal to be embodied with it, and cannot be again diflodged but by employing fire. I believe thefe combinations frequently occur in nature, and the great Newton fufpected it.

and yields a portion of the abforbed air, which is a compound of azote and carbonic acid gafes;

47th. I dried the pieces of charcoal which had been wetted in the preceding experiment, in the air: I afterwards examined them, by again heating them to rednefs in the fire, and placing them in the machine; they abforbed the fame quantity of air as before.

by which it regains its abforbent power.

No water produced by the diminution of the gas under examination.

48th. I directed all my attention, as well after as during the courfe of the experiments on atmofpheric air, to fee if I could perceive any drops of water either in the tube or in the apparatus, but I did not obferve the leaft trace of it; neither was the charcoal moift; on the contrary, its furface was covered with afhes.

Experiments to be made by faturating the Charcoal with different Subftances.

Experiments ftill to be made.

49th. Since, during the progrefs of an inveftigation, there are always new experiments which offer themfelves, I perceived that it would be interefting to make trial with pieces of charcoal faturated with different faline and acid fubftances, as M. M. Rouppe and Van-Norden, of Rotterdam, had undertaken to do, and to fee the effects they would produce, with my machine, as well in atmofpheric air as in the different gafes: But my memoir being of fufficient length already, I leave the care of thefe enquiries to other philofophers; and this the more willingly, fince it appears that one of the moft able chemifts, M. Van Mons, has engaged in this fubject, which, in his hands, will doubtlefs receive every degree of perfection which can be defired.

50th. I fhall only give the refult of four experiments which I made with atmofpheric air.

I faturated pieces of charcoal with nitric acid, fulphuric acid, folution of potafh, and lime-water; they were not ufed until

until they had been thoroughly dried on filtering-paper. I put them cold into my machine. The following are the refults:

1ft. In the firft fix hours the charcoal, faturated with nitric acid, produced an abforption of four lines and a half; afterwards it gave back the air, and even depreffed the mercury below its level.

Effects of charcoal faturated with nitric acid;

2d. The charcoal, faturated with fulphuric acid, effected an abforption of nine lines, which it preferved uniformly for twenty-four hours.

with fulphuric acid;

3d. The charcoal, faturated with folution of potafh, made an abforption of three lines, and remained fo for twenty-four hours.

with folution of potafh; and

4th. That, faturated with lime-water, made only a fmall abforption of a line and a half, although it was kept in experiment for twenty-four hours.

with lime-water.

Thefe four experiments prove that charcoal, faturated with different fubftances, lofes, in part, its power of abforbing: probably thefe folutions, by filling the pores of the charcoal, in fome meafure deprive it of this property.

Charcoal lofes its abforbent power by being faturated with fome fubftances.

51ft. No perfon can be more aware than I am, that my enquiry is far from being carried to that degree of perfection which I wifhed: I hope, neverthelefs, that philofophers will not be diffatisfied that I have prefented them with new experiments, and have made them acquainted with an inftrument, by means of which they can appreciate the effects of folar light and caloric on charcoal; an inftrument which, in many cafes, will ferve them for an eudiometer, and which, when azote fhall be better known, will acquire greater precifion. I hope that, by the labours of other philofophers who may employ themfelves on this fubject, this interefting branch of natural philofophy will be brought to perfection, and an explanation of thefe furprifing phenomena be obtained.

VI.

Farther Observations on the Constitution of mixed Gases. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

An apology for this letter.

OTHER philosophers, besides Mr. Dalton, have maintained the existence of free elastic vapour in the atmosphere; but as the modification given by this gentleman to the opinion, is perhaps the last it will receive, I am desirous of trespassing once more on your pages, in reply to my opponent, as well as to shew the changes which the present controversy has produced in his system.

Some objections stated by Mr. D. considered.

Mr. Dalton's last letter begins by exposing my ignorance of chemistry, and disapproving the data, by help of which air has been shewn to be a chemical compound. Although his suspicions may be just in many instances, they are wrong in this; for the facts stated by Mr. D. were known to me at the time of writing that paper; and the reprobated data were adopted for the following reasons:

Specific gravity of oxigenous gas.

Mr. Davy found the weight of 100 cubic inches of oxigenous gas, of the temperature of 50°, to be 35.06 grains, the barometer standing at 30 inches: now the densities of the same elastic fluid at 50° and 60°, are in the ratio of 105824 to 103744 *: but when the magnitude is given, *i. e.* 100 parts, the weights are as the densities; consequently the weight of 100 cubic inches of oxigenous gas at 60°, is 34.37 grains, the barometer standing at 30: but 31 grains being the weight of an equal bulk of air in like circumstances, and its specific gravity being denoted by 1000, the corresponding specific gravity of oxigenous gas is expressed by the number 1108; which exceeds Mr. Kirwan's expression only by five. This approximation to an exact coincidence, affords the strongest evidence of the correctness both of Mr. Kirwan and Mr. Davy, that can be expected in a question of the kind; and, as the former gentleman made use of the same apparatus to determine the weight of air which had lost its oxigen by being exposed to

* Thomson's Chemistry, Vol. I. p. 342.

the

the martial paſte of ſulphur, the ſpecific gravity, drawn from his experiment, was certainly to be preferred in my calculation to any other. This reſiduum was called by me azote, probably becauſe Mr. Kirwan called it phlogiſticated air: but it was evidently conſidered as a compound in the eſſay alluded to by Mr. Dalton; and certainly the advocate for a multiplicity of co-exiſtent atmoſpheres will not controvert the ſuppoſition. The remaining objections made by my opponent to the paper in queſtion, are levelled at my ignorance, not at my concluſions; they ſhall therefore be paſſed over in ſilence. In the mean time, Mr. D. perhaps will not take it amiſs if I requeſt him to repeat his calculations with the ſpecific gravity of oxigenous gas when properly corrected, and to try what will be the variation of the eudiometer, either on the data of Mr. Davy or M. Lavoiſier.

A new inſtrument recommended.

Permit me, Mr. Nicholſon, to conclude this part of my ſubject by obſerving, that an inſtrument of great uſe in pneumatics might be conſtructed on the principles of your hydroſtatic weighing machine. The body of it ſhould be of glaſs, furniſhed with proper ſtopples and ſtop-cocks, ſo that it might be eaſily exhauſted and repleniſhed. The preceding is but an imperfect ſketch of an inſtrument, the mechaniſm of which I reſign to your pen and pencil, ſhould the idea appear worth purſuing.

A miſrepreſentation pointed out.

Mr. Dalton's ſtrictures on my laſt letter aſcribe to me very ſingular opinions; but he neglects to point out the paragraph where the grounds of this charge may be found; a charge which inſinuates, that, according to my notions of things, a veſſel containing a cubic foot of one gas cannot receive an equal bulk of a different kind: Here indeed he accuſes me of groſs ignorance; for there are but few who do not know that fermenting liquors increaſe the denſity of the gaſes, which occupy part of the cloſe veſſels containing them. In reality, the charge is ſo perfectly groundleſs, that, had it been made by almoſt any other perſon, I would not have heſitated to have pronounced my accuſer guilty of miſrepreſentation to conceal his want of argument, and to ſecure the prejudices of the ſuperficial reader in his own favour. He may court the admiration of ſuch; I do not deſire their applauſes.

Mr. D.'s important argument no argument at all.

Mr. Dalton ſpeaks with confidence of what he calls his important argument, *i. e.* his aſſertion, that equal quantities of vapour

vapour enter a vacuum and an equal ſpace previouſly occupied by a gas. No one would refuſe its due importance to this argument, after ſeeing or peruſing a ſatisfactory demonſtration; but I know of no ſuch thing in Mr. Dalton's eſſays. He aſſerts, indeed, that a quantity of dry air, confined in a tube, receives an augmentation of force from the preſence of water, equal to that which is produced by the ſame cauſe in the exhauſted tube. But this is no demonſtration, unleſs the poroſity of air be firſt admitted; for, without this conceſſion, Mr. D. has no right to eſtimate the quantity of vapour preſent by the ſpring of it; this ought to be determined by weight only. But when did Mr. D. weigh an exhauſted veſſel, and afterwards take its weight when filled with vapour? When has he repeated the ſame experiment upon the ſame veſſel containing dry and moiſt air, and found the reſult favourable to his hypotheſis? Negative anſwers to theſe queſtions muſt convince an impartial judge, that the poroſity of the air has been again artfully introduced to eſtabliſh its own exiſtence: thus we find the fundamental datum of the ſyſtem to be demonſtrated by arguing in a circle, which begins and ends with the thing to be proved.

On the other hand, Mr. D. is convinced that the admiſſion of vapour into an open bottle expels part of the air. This is a proof that ſteam, ſuppoſing its exiſtence in the atmoſphere, finds a ſpace crowded with gaſes leſs convenient than a vacuum; it, therefore, makes room for itſelf, by diminiſhing the denſities of theſe gaſes, and creates in them Mr. Dalton's capacity for the reception of water. This fact being eſtabliſhed, it will alſo be evident, that, when vapour is forced into a gas confined in a cloſe veſſel, the former cannot make its way through the latter, otherwiſe than by condenſing it: Such is the nature of the pores of the air, according to Mr. D.'s own principles. The atmoſphere is alſo found to oppoſe a ſimilar reſiſtance to newly developed gaſes; for the gaſeous compound contained in an open veſſel filled in part with a fermenting liquor, is not a column of the atmoſphere permeated by an additional body of the carbonic acid, but a mixture, in which the proportion of common air is comparatively ſmall. This fact overturns Mr. D.'s general concluſion, that atmoſpheres may be added to the compound atmoſphere at pleaſure.

ſure, or taken from it, without its diſturbing the denſities and ſituations of the remaining atmoſpheres *.

Change in Mr. D.'s opinions.

The free paſſage of gaſes through each other was once the fundamental maxim of my opponent; at which time he maintained, that the variations in the weight of the atmoſpheric compound ariſe from the changeable nature of the aqueous atmoſphere, not from the permanent gaſes †. This dogma is eſſential to the ſyſtem; it has, however, been abandoned by its author himſelf; for Mr. D. when attempting to explain the obſervation of Mr. Kirwan, is obliged to admit the permanent gaſes to be more abundant in dry than in moiſt air; that is, the denſities and poſitions of theſe gaſes depend upon the ſtate of the contained vapour, contrary to the hypotheſis. This departure from the primitive maxim is avowed openly in Mr. D.'s explanation of the experiment with the moiſt bottle. Here he is compelled to confeſs, that ſteam, at low temperatures, diſtends the pores of air; and endeavours to preſerve the exiſtence of his aqueous atmoſphere by ſomething, which looks like a demonſtration from the final Q. E. D.: but it ought to be remembered, that the particles of a fluid preſs equally in all directions; conſequently, that the corpuſcles of air would act with their full force on the contiguous corpuſcles of his ſuppoſed vapour.

Perhaps Mr. D. will perceive, by this time, that logical preciſion has placed him in the circumſtances of certain philoſophers mentioned in his third eſſay, who are unable to defend their opinions. In fact, Mr. D.'s hypotheſis is repugnant to natural appearances in its primitive form; for, according to it, a ſhower of rain is a prodigy; ſeeing the drops muſt diſplace equal bulks of air, and this removal muſt be brought about by the inadequate weight of a column of vapour which has loſt part of its ſpring.

JOHN GOUGH.

Middleſhaw, December 13, 1804.

P. S. Since the publication of my paper on vegetation, I have found, by Dr. Thomſon's Chemiſtry, that M. Ingenhouz had formerly made the ſame diſcovery. In 1795, I read

* Mancheſter Memoirs, Vol. V. p. 546.

† Mancheſter Memoirs, Vol. V. p. 547.

his

his work, *Sur les Vegetaux*, printed at Paris, 1787; but undoubtedly this ſection had by ſome means been overlooked. All that I have to do at preſent, by way of reparation, is to thank Dr. Thomſon for his information, and to declare that paper of no value; which in all probability will prove a ſufficient apology to M. Ingenhouz.

VII.

On two Metals, found in the black Powder remaining after the Solution of Platina. By SMITHSON TENNANT, *Eſq. F.R.S. From the Philoſophical Tranſactions for* 1804.

Introduction, with reference to the experiments made by Deſcotils,

UPON making ſome experiments, laſt ſummer, on the black powder which remains after the ſolution of platina, I obſerved that it did not, as was generally believed, conſiſt chiefly of plumbago, but contained ſome unknown metallic ingredients. Intending to repeat my experiments with more attention during the winter, I mentioned the reſult of them to Sir Joſeph Banks, together with my intention of communicating to the Royal Society, my examination of this ſubſtance, as ſoon as it ſhould appear in any degree ſatisfactory. Two memoirs were afterwards publiſhed in France, on the ſame ſubject; one of them by M. Deſcotils, and the others by Meſſrs. Vauquelin and Fourcroy. M. Deſcotils chiefly directs his attention to the effects produced by this ſubſtance on the ſolutions of platina. He remarks, that a ſmall portion of it is always taken up by nitro-muriatic acid, during its action on platina; and, principally from the obſervations he is thence enabled to make, he infers, that it contains a new metal, which, among other properties, has that of giving a deep red colour to the precipitates of platina.

and by Vauquelin.

M. Vauquelin attempted a more direct analyſis of the ſubſtance, and obtained from it the ſame metal as that diſcovered by M. Deſcotils. But neither of theſe chemiſts have obſerved, that it contains alſo another metal, different from any hitherto known.

Subſtance obtained from the grains of platina,

The ſubſtance with which my experiments were made, was obtained from platina which had been previouſly freed from the ſand and other impurities generally mixed with it; ſo that

it

it muſt have been contained in the ſubſtance of the grains of platina. Though it has ſomewhat the appearance of plumbago, it may eaſily be diſtinguiſhed by its ſuperior weight. By weighing it in a phial with water, I found its ſpecific gravity almoſt 10.7.

reſembling plumbago, but heavier.

Before I deſcribe the method of ſeparating the two metals of which it conſiſts, it may be worth while to mention the effects of it, when combined with different metals in its entire ſtate. It readily unites with lead; but, even with ten times its own weight, the compound has not, when melted, much fluidity. Upon diſſolving the lead in nitrous acid, the black powder was obtained, with little apparent alteration, not having been entirely broken down, but conſiſting chiefly of the ſame ſcaly particles as at firſt. With biſmuth, zinc, and tin, the effects were nearly ſimilar; but, by fuſion with copper in a very ſtrong heat, a more perfect union was produced. On attempting to diſſolve the compound by nitro-muriatic acid, ſome of the powder was taken up with the copper, forming a very dark ſolution.

It combines with metals:—A ſmall portion renders lead, biſmuth, zinc, and tin, difficultly fuſible

with copper;

The undiſſolved portion conſiſted partly of the ſubſtance in its original form of ſcales, and partly of a blacker powder, the particles of which were too ſmall to be viſible, and which had probably been completely combined with the copper. This ſubſtance may be eaſily united, by fuſion, with ſilver or gold; and it is particularly deſerving of attention, that it cannot be ſeparated from theſe metals, by the uſual proceſs of refining. It remains combined with either of them, after cupellation with lead; and with the gold, after quartation with ſilver. The alloys retain conſiderable ductility; and the colour of that with gold, is not materially different from pure gold.

ſilver or gold; not ſeparable by refining.

I ſhall now proceed to deſcribe the analyſis of the black powder, and the properties of the two metals which enter into its compoſition. The method which I uſed for diſſolving it, was ſimilar to that employed by M. Vauquelin, the alternate action of cauſtic alkali, and of an acid. I put a quantity of the powder into a crucible of ſilver, with a large proportion of pure dry ſoda, and kept it in a red heat for ſome time. The alkali being then diſſolved in water, had acquired a deep orange, or browniſh-yellow colour, but much of the powder remained undiſſolved. This powder, digeſted in marine acid, gave a dark blue ſolution, which afterwards became of a duſky

Analyſis of the black powder; which conſiſts of two metals.

Fuſion with ſoda.

Solution in water, deep orange, leaving a powder;

which was partly diſſolved in muriatic acid.

dufky olive-green, and finally, by continuing the heat, of a deep red colour. Part of the powder being yet undiffolved by the marine acid, was heated as before with alkali; and, by the alternate action of the alkali and acid, the whole appeared capable of folution. At each operation, fome filex was taken up by the alkali; and, as this continued till the metallic part was entirely diffolved, it feems to have been chemically combined with it.

Fufion, &c. repeated on the undiffolved refidue.

Silex.

The alk. folution contains an oxide which feparates by repofe.

The alkaline folution contains the oxide of a volatile metal, not yet noticed, but which I fhall prefently defcribe, and alfo a fmall proportion of the other metal. If this folution is kept for fome weeks, the latter metal feparates fpontaneoufly from it, in the form of very thin flakes, of a dark colour.

The acid fol. contains the fame metal, but principally that examined by Vauquelin.

The acid folution alfo contains both the metals, but principally that which has been mentioned by the French chemifts. The properties of this laft metal, which they have remarked, are thofe of giving a red colour to the triple falt of platina with fal-ammoniac, of not being altered by muriate of tin, and of giving, with pure alkali, a dark brown precipitate. M. Vauquelin alfo adds, that it is precipitated by galls, and by pruffiate of potafh; but I fhould rather afcribe thefe precipitates to fome impurity, and probably to iron.

The name *Iridium* given to the metal examined by Vauquelin.

As it is neceffary to give fome name to bodies which have not been known before, and moft convenient to indicate by it fome characteriftic property, I fhould incline to call this metal *Iridium*, from the ftriking variety of colours which it gives, while diffolving in marine acid.

In order to obtain the compound of this metal with marine acid in a pure ftate, I tried to make it cryftallize.

Cryftals of its muriate

diffolved in water.

By flow evaporation of the folution, only an imperfectly cryftallized mafs was produced; but this, being dried on blotting-paper, and diffolved in water, afforded, by again evaporating as before, diftinct octaedral cryftals. Thefe cryftals diffolved in water, gave a deep red coloured folution, inclining to orange. With an infufion of galls no precipitate was formed, but the colour was inftantly, and almoft intirely, taken away. Muriate of tin, carbonate of foda, and pruffiate of potafh, produced nearly the fame effect. Pure ammonia precipitates the oxide; but (poffibly from adding it in excefs) I found it retained a part in folution, acquiring a purple colour. The pure fixed alkalis alfo precipitate the greater part of the oxide,

Precip. by alkalis and metals.

oxide, but are capable of retaining a part in ſolution, becoming of a yellow colour. All the metals which I tried, excepting gold and platina, produced a dark or black precipitate from the muriated ſolution, which is at the ſame time deprived of its colour. The iridium may be obtained in a pure ſtate, merely by expoſing the octaedral cryſtals to heat, which expels the oxigen and the muriatic acid. It appeared of a white colour, and was not capable of being melted, by any degree of heat I could apply. I could not combine it with ſulphur, nor with arſenic. Lead eaſily unites with it; but is ſeparated by cupellation, leaving the iridium upon the cupel, as a coarſe black powder. Copper forms with it a very malleable alloy, which, after cupellation with the addition of lead, left a ſmall proportion of the iridium, but much leſs than in the former caſe. Silver may be united with it, and the compound remains perfectly malleable. The iridium was not ſeparated from it by cupellation, but occaſioned on the ſurface a dark or tarniſhed hue. It appeared not to be perfectly combined with the ſilver, but merely diffuſed through the ſubſtance of it, in the ſtate of a fine powder. Gold alloyed with iridium is not freed from it by cupellation, nor by quartation with ſilver. The compound was malleable; and did not differ much in colour from pure gold, though the proportion of alloy was very conſiderable. If the gold or ſilver is diſſolved, the iridium is left, in the form of a black powder.

The metal iridium is white, infuſible, and rejects ſulphur and arſenic.

Its habitudes with the metals.

The yellow alkaline ſolution, which I have already mentioned as containing a metallic oxide, diſtinct from the former, is conſidered by M. Vauquelin as a ſolution of the oxide of chrome in alkali; but I could not, by any teſt, diſcover the preſence of chrome. After the ſuperfluous alkali had been neutralized by an acid, it produced a pale or buff-coloured precipitate with a ſolution of lead, and not the bright yellow which is given by chrome. But, as we are indebted to the above diſtinguiſhed chemiſt, among many other important diſcoveries, for our knowledge of the exiſtence of chrome, it is not improbable that ſome kinds of platina may contain that ſubſtance, beſides the other bodies uſually mixed with it. When the alkaline ſolution is firſt formed, by adding water to the dry alkaline maſs in the crucible, a pungent and peculiar ſmell is immediately perceived. This ſmell, as I afterwards

The alkaline ſolution did not contain *Chrome.*

It contains the volatile oxide of a metal called *Oſmium*;

wards discovered, arises from the extrication of a very volatile metallic oxide; and, as this smell is one of its most distinguishing characters, I should on that account incline to call the metal *Osmium*.

expellable from the alkali by an acid;

This oxide may be expelled from the alkali by any acid, and obtained in solution with water by distillation. The sulphuric acid, being the least volatile, is the most proper for this purpose; but as, even of this acid, a little is liable to pass over, a second slow distillation is required, to obtain the oxide perfectly free from it. The solution thus procured is without colour, has a sweetish taste, and the strong smell before mentioned. Paper stained blue with violets, was not changed by it to red; but, by being exposed to the vapour of it in a phial, the paper lost much of its blue colour, and inclined to gray. As a certain quantity of this oxide is extricated during the solution of the iridium in marine acid, that part may also be obtained by distillation.

or obtained by distilling the black powder with nitre.

Another mode by which the oxide of osmium may be obtained in small quantity, but in a more concentrated state, is, by distilling with nitre the original black powder procured from platina.

Process described.

With a degree of heat hardly red, there sublimes into the neck of the retort, a fluid apparently oily, but which, on cooling, concretes into a solid, colourless, semitransparent mass. This, being dissolved in water, forms a solution similar to that before described. The oxide, in this concentrated state, stains the skin of a dark colour, which cannot be effaced.

Most striking character of solution of osmium:

The most striking test of the oxide of osmium, is an infusion of galls, which presently produces a purple colour, becoming soon after of a deep vivid blue. By this means, the presence of this, and of the metal first described, may be observed,

Habitudes with various bodies;

when the two are mixed together. The solution of iridium is not apparently altered by being mixed with the oxide of osmium; but, on adding an infusion of galls, the red colour of the first is instantly taken away, and soon after the purple and blue colour of the latter appears. The solution of the oxide of osmium with pure ammonia, becomes somewhat yellow, and slightly so with carbonate of soda. It is not affected by pure magnesia, nor by chalk; but with lime a solution is formed, of a bright yellow colour. The solution with lime gives with galls a deep red precipitate, which becomes

becomes blue by acids. It produces no effect on a solution of platina or gold; but precipitates lead of a yellowish-brown, mercury of a white, and muriate of tin of a brown colour.

The oxide of osmium becomes of a dark colour with alcohol, and after some time, separates in the form of black films, leaving the alcohol without colour. The same effect is produced by ether, and much more quickly. — alcohol;

This oxide appears to part with its oxygen to all the metals, excepting gold and platina. Silver being kept in a solution of it for some time acquires a black colour; but does not entirely deprive it of smell. Copper, tin, zinc, and phosphorus, quickly produce a black or gray powder, and deprive the solution of all smell, and of the power of turning galls of a blue colour. This black powder, which consists of the osmium in a metallic state and the oxide of the metal employed to precipitate it, may be dissolved in nitro-muriatic acid, and then becomes blue with infusion of galls. — the metals.

Amalgam with mercury.

If the pure oxide of osmium, dissolved in water, is shaken with mercury, it very soon loses its smell; and the metal, combining with the mercury, forms a perfect amalgam.

Pure osmium had by distilling off the mercury.

Much of the mercury may be separated by squeezing it through leather, which retains the amalgam of a firmer consistence. The remaining mercury being distilled off, a powder is left, of a dark gray or blue colour, which is the osmium in its pure state. By exposing it to heat with access of air, it evaporates, with the usual smell; but, if the oxidation is carefully prevented, it does not seem in any degree volatile. Being subjected to a strong white heat, in a cavity made in a piece of charcoal, it was not melted, nor did it undergo any apparent alteration. Heated in a similar situation with copper and with gold, it melted with each of these metals, forming alloys which were quite malleable. These compounds were easily dissolved in nitro-muriatic acid, and, by distillation, afforded the oxide of osmium with the usual properties.

It is gray, and not fusible; nor volatile, unless oxided.

Alloys.

Pure osmium not soluble in acids; but easily with alkali.

The pure metal which has been previously heated, does not seem to be acted on by acids; at least I could not perceive any effect produced by boiling it for some time with nitro-muriatic acid. By heating it in a silver cup with caustic

alkali,

alkali, it immediately combined with the alkali, and, with water, gave a yellow folution, fimilar to that from which it was procured. Acids expel from this folution the oxide of ofmium, which has the ufual fmell, and the power of giving to infufion of galls the blue colour before mentioned.

VIII.

Remarks upon certain Obfervations of Mr. WILKINSON, *refpecting Galvanifm. By* RA. THICKNESSE, *Efq.*

To Mr. NICHOLSON.

SIR, *Wigan, Dec.* 17, 1804.

Obfervations refpecting galvanifm, &c.

THE remarks of Mr. Wilkinfon (which I have not had an earlier opportunity of attending to) on my letter of the 20th September, on galvanifm, are in general founded only on mifconception; the confequence, probably, of a hafty perufal in a public room. He attributes to me an affertion, " that two metals are requifite to the production of galvanic phenomena;" but my words are, and particularly marked by italics, " two metals, *or other fubftances;*" and were intended to fignify two metals, or two other fubftances; or one metal and one other fubftance; and to include even that which may be diffolved in the water. The experiment, therefore, of La Grave, with brain and mufcle, which Mr. W. adduces, is not an exception; and " that a fingle metal fuffices" perfectly with pure water only, he did not, I fancy, wifh to be underftood.

That copper and the other negative metals are, as I expreffed, " acted upon by the hidrogen," I muft refer to Mr. W. for authority, to pages 177 and 178 of your Journal; and that particularly filver (my words are copper, or filver) is rendered more brittle, to page 85: but there cannot, furely, be ftronger evidence that copper has an affinity for hidrogen, and confequently an influence in the decompofition of the water, than is afforded by the experiments in which hidrogen gas is obtained from the copper of the pile, and oxigen gas from the oxidated zinc; part of the hidrogen, and of the oxigen, being prevented from uniting with, and borne away from the metals by the electricity.

Mr.

Mr. W. imputes to me alfo the miftake of having "obferved that the fenfation is in proportion to the furface acted on;" adding, that "the experiments of the French philofophers proved, that the action of galvanifm on animal fubftances is in the ratio of the number of plates employed, and not the furfaces expofed." This is another inftance of the inaccuracy of Mr. W.'s obfervations; for fenfation I never mentioned; nor action on animal fubftances; not at all confidering the intenfity, but the quantity only of the electric fluid produced; which may not always correfpond; for the charge of fmall plates being weak, although according to their furfaces, the electricity meets with almoft infurmountable obftructions from the interftices of the pile; which to a ftronger charge, from larger plates, are very trifling impediments: but Mr. W. thinks, himfelf, that "the production of galvanic phenomena is always proportionate to the degree of oxidation;" and muft not the quantity of metal oxidated be in proportion to the furface expofed to the water? The French philofophers, too, have "concluded (I quote Mr. W.'s own words, from p. 207 of your laft volume) that the effects of a galvanic battery on metallic fubftances, are in proportion to the furfaces of the plates employed:" Muft we not then fufpect the experiments to have been imperfect which led them to conclude, if they have fo concluded, that the effects on animal fubftances are different? Obfervations refpecting galvanifm, &c.

Whether I "fet out too hypothetically as to electricity being a modification of caloric," is a queftion on which many of the moft celebrated men of fcience would differ from Mr. W. in opinion, "*Pars invenit utraque Cauffas;* yet it appears to me, that the experiment noticed by *a Correfpondent*, p. 173, almoft determines it: "If the gas which is produced from one of the wires communicating with the pile in the water, be united and inflamed with the other in a juft proportion, the water which is common to both is reproduced, and *common fire* in great abundance." Thus the electric fluid, which alone conveys the oxigen and hidrogen from the water, actually becomes, when differently modified by combuftion, for there is "no evidence of the prefence of fire until this point of time," active caloric.

But fuppofing, with Mr. W. that the electric matter and caloric are perfectly diftinct bodies, we have ftill the fame

reafon

Obfervations refpecting galvanifm, &c.

reafon to conclude that the former is combined with water, which we have to believe that it is contained in metals: What then becomes of it when the water is decompofed, if, as he imagines, the electric, or "galvanic phenomena, entirely depend upon the metal undergoing a chemical change," I muft confefs I cannot conjecture: but, in truth, it is entirely the caufe of all the phenomena which take place: the quantity afforded by the metal, fo very fmall a portion of which undergoes any chemical change, muft be too trifling to be confidered; for if any more than the oxidated part of the metal contributed, the quantity of the metal in the plates, as well as the extent of its furface, would in fome degree have an effect: but this, as I alledged, is not the cafe; and, therefore, Mr. W.'s fanciful conjecture, that there is as it were a fpring of electric fluid in the metal, as a refource for a fupply, although very ingenious, may be regarded as a perfect hypothefis—a fuppofition unfupported by any fact; and as an inftance, from his partiality for it on this occafion, of our difpofition to overlook imperfections in our own performances, whilft we obferve them in the works of others.

I am, Sir,

Your moft obedient fervant,

RA. THICKNESSE.

IX.

Abftract of a Memoir on the Poffibility of obtaining Pruffiate of Potafh free from Iron; the Unalterability of the Pruffic Acid at high Temperatures; and the true Nature of the Combinations of this Acid with different Bafes. By BUCHOLZ.

(Concluded from p. 282, Vol. IX.)

General deductions.

THESE two experiments therefore prove, that from four parts of dried blood and one of carbonate of potafh, no more pruffiated alkali can be obtained than from two of blood and one of alkali.

From what has been fo far ftated the author concludes, after varying and repeating the experiments here detailed,

1. That

1. That four parts of blood to one of carbonate of potash are too much, and that equal parts of blood and carbonate of potash are rather good proportions for obtaining the most possible quantity of prussiated alkali. The latter proportions have, moreover, the disadvantage, that the excess of alkalis acts upon the oxide of iron of the blood as well as upon the crucible, and therefore introduces new impurities. Two parts of blood and one of carbonate of potash, appear to be a better proportion for obtaining the greatest possible quantity of prussiate of potash, under like circumstances.

Best proportion of carbonate of potash and dried blood, are 1 p. and 2 b.

2. Alcalies deprived of carbonic acid, are not better than carbonated alkalies for the production of prussiated alkalies by treatment with blood; for they become reconverted into carbonated alkalies during the process.

It is no advantage to use a pure alkali.

3. No pure prussiate of potash can be obtained, except care be taken that the mixture of blood and alkali be neither too much nor too little ignited. The criterion which may serve as a guide to the operator, is the cessation of the flame, after a gradually increased ignition without fusion. For if the mass be heated suddenly so as to effect a partial fusion, or if it be continued after the disappearance of the flame, it will be in vain to look for pure prussiated alkali.

The formation of pure prussiate requires a gradual heat without fusion, and no longer than till the flame ceases.

4. The high colour of prussiated alkalis is not always owing to the presence of iron, but more frequently to a minute quantity of charcoal. This impurity is not capable of yielding results that could mislead the chemical enquirer in the application of this re-agent.

When the high colour of p. alk. arises from charcoal, it does not produce fallacy.

5. The quantity of water employed for the lixiviation of prussiated alkali from the ignited mass, should be as small as possible, and heat should be avoided as much as possible. For if prussiated alkalis be heated in contact with water, part of the prussic acid is liberated, ammonia is produced, and carbonate of potash formed.

Very little water should be used, and heat avoided.

6. Acetic acid cannot be employed for removing the admixture of carbonate of potash from prussiated alkalis; for the energy of the affinity of prussic acid is less than the energy of the affinity of carbonic acid to potash; the union of the prussic acid and potash is therefore demolished, in preference to that of the carbonate of potash.

Acetic acid cannot be used to take off the redundant potash.

X.

On a new Metal, found in Crude Platina. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S. From the Philofophical Tranfactions,* 1804.

Introduction.

NOTWITHSTANDING I was aware that M. Defcotils had afcribed the red colour of certain precipitates and falts of platina, to the prefence of a new metal; and although Mr. Tennant had obligingly communicated to me his difcovery of the fame fubftance, as well as of a fecond new metal, in the fhining powder that remains undiffolved from the ore of platina; yet I was led to fuppofe that the more foluble parts of this mineral might be deferving of further examination, as the fluid which remains after the precipitation of platina by fal ammoniac, prefents appearances which I could not afcribe to either of thofe bodies, or to any other known fubftance.

New metal, *Rhodium.*

My inquiries having terminated more fuccefsfully than I had expected, I defign in the prefent Memoir to prove the exiftence, and to examine the properties, of another metal, hitherto unknown, which may not improperly be diftinguifhed by the name of *Rhodium,* from the rofe-colour of a dilute folution of the falts containing it.

Palladium found in ore of platina.

I fhall alfo take the fame opportunity of ftating the refult of various experiments, which have convinced me, that the metallic fubftance which was laft year offered for fale by the name of Palladium, is contained (though in very fmall proportion) in the ore of platina.

Remaining folution after precipitation of platina, contains iron and other metals.

The colour of the folution that remains after the precipitation of platina, varies, not only according to its ftate of dilution, but alfo according to the ftrength and proportions of the nitric and muriatic acids employed. This colour, though principally owing to the quantity of iron contained in it, arifes alfo in part from a fmall quantity of the ammoniaco-muriate of platina, that neceffarily remains diffolved, and from other metals contained in ftill fmaller proportions.

Precipitable by zinc or iron.

(A. 1.) To recover the remaining platina, as well as to feparate the other metals that are prefent from the iron, I have in fome experiments employed zinc, in others iron, for their precipitation. The former appears preferable; but, when the latter has been ufed, the precipitate may immediately be freed from

rom the iron that adheres to it, by muriatic acid, without the lofs of any of thofe metals which are at prefent the fubject of iniquiry.

(A 2.) Having in one inftance diffolved fuch a precipitate in nitro-muriatic acid, and precipitated the platina by fal ammoniac, I fuffered the remaining fluid to evaporate without heat; and obtained a mixture of various cryftals, very different from each other in form and colour. From thefe, I felected for examination fome that were of a deep red colour, partly in thin plates adhering to the fides of the veffel, and partly in the form of fquare prifms having a rectangular termination.

The fame refidual folution affords cryftals by fpontaneous evap.

(A 3.) A portion of thefe cryftals being heated in a fmall tube, yielded fal ammoniac by fublimation, and left a black refiduum, which, by greater heat, acquired a brilliant metallic whitenefs, but could not be fufed under the blowpipe. Having obtained this fubftance from a diftinctly cryftallized falt, I was inclined to confider it as a fimple metal; and, as I found it to be wholly infoluble in nitro-muriatic acid, I judged it not to be platina.

Deep red cryftals left a white metal by heat, —not platina.

(A 4.) The cryftals alfo, inftead of being nearly infoluble, like the ammoniaco-muriate of platina, were diffolved in a fmall quantity of water, and gave a rofe-coloured folution. Upon mixing this with a folution of platina, the ammonia was transferred by fuperior affinity to the latter, forming an ammoniaco-muriate of platina; and the precipitate was of a yellow colour. Confequently, the metal contained in the falt, was neither platina nor that which gives the red colour to the falts of platina.

The cryftals were not of platina, nor that which makes the falts of platina red.

It would be ufelefs to detail my firft unfuccefsful experiments made upon the properties of this metal, in hopes of difcovering means by which its feparation from platina might be effected; I fhall therefore confine myfelf to the following procefs, which appears to be the moft direct for procuring rhodium in a ftate of purity. In the fame procefs alfo palladium is obtained, fo as to afford a prefumption, that it is rather a natural fimple body, than any artificial compound.

The bafe was the metal rhodium now firft noticed.

(B 1.) Since the platina to be procured in this country, generally contains fmall fcales of gold intermixed, as well as a portion of the mercury which the Spaniards employ for the feparation of the gold, the platina ufed for my experiments, after being by mechanical means freed, as far as poffible, from

Procefs for obtaining it.
Purification of the platina-grains.

all

all visible impurities, was exposed to a red heat, for the purpose of expelling the mercury. It was then digested for some time in a small quantity of dilute nitro-muriatic acid, and frequently shaken, till the whole of the gold was dissolved, together with any impurities that might superficially adhere to the grains of platina.

Solution of the ore in nitro-mur. acid.

(B 2.) Of the ore thus prepared, nearly 2½ ounces were then dissolved in nitro-muriatic acid, (diluted for the purpose of leaving as much as possible of the shining powder,) and the whole suffered to remain in a moderate sand heat, till completely saturated.

Precipitation by sal amm.

(B 3.) Such a portion of this solution was then taken for analysis, as corresponded to 1000 grains of the prepared ore. An ounce of sal ammoniac was next dissolved in hot water, and used for the precipitation of the platina. The precipitate obtained was of a yellow colour, and, upon being heated, yielded 815 grains of purified platina.

Residual liquor precipitated by zinc.

(B 4.) The water used for washing this precipitate having been added to the solution poured from it, a piece of clean zinc was immersed in it, and suffered to remain, till there appeared to be no further action upon the zinc. The iron contained in the ore (to the amount of 14 or 15 *per cent.*) remained in solution. The other metals had subsided, in the form of a black powder, which I estimated between 40 and 50 grains; but, as there was no occasion to weigh it with accuracy, I thought it better not to dry this precipitate, for, if it be heated, the rhodium is in danger of being rendered insoluble.

Precipitate deprived of copper and lead by nitric acid; and then dissolved (most part) in nitro-m. acid.

(B 5.) As I had previously ascertained that this precipitate would contain platina, rhodium, the substance called palladium, copper, and lead, the two last metals were first dissolved in very dilute nitric acid, aided by a gentle heat. The remainder, after being washed, was digested in dilute nitro-muriatic acid, which dissolved the greater part, but left as much as 4½ grains undissolved.*

Addition of common salt and evap. left soda-muriates of platina, palladium and rhodium. All but the triple salt of rhodium was washed off by alcohol.

(B 6.) To the solution were added 20 grains of common salt; and, when the whole had been evaporated to dryness with a very gentle heat, the residuum, which I had found, from prior

* It was presumed that this residuum consisted principally of the metal called by Mr. Tennant Iridium; but, as it was accidentally mislaid, and was not examined, it might also contain a portion of rhodium.

experi-

experiments, would confift of the foda-muriates of platina, of palladium, and of rhodium, was wafhed repeatedly with fmall quantities of alcohol, till it came off nearly colourlefs. There remained a triple falt of rhodium, which by thefe means is freed from all metallic impurities.

(C 1.) This falt, having been diffolved in a fmall quantity of hot water, and let to ftand 12 hours, formed rhomboidal cryftals, of which the acute angle was about 75°. Solution in water.

(C 2.) It was then again diffolved in water, and divided into two equal portions. Of thefe, one was decompofed by a piece of zinc, and the other examined by the following re-agents. One half precip. by zinc.

(C 3.) Sal ammoniac occafioned no precipitation; but, when a folution of platina was added to the mixture, a precipitate was immediately formed, and the colour of this precipitate was yellow; which again proves that the metal contained in this falt, is neither platina itfelf nor that which gives the red colour to its precipitates. The other half was precip. by platina, but not by fal-amm.

(C 4.) Pruffiate of potafh occafioned no precipitation, as it would have done, if the folution had contained palladium. nor by pr. potafh,

(C 5.) Hydro-fulphuret of ammonia, which would have precipitated either platina or palladium, caufed no precipitation of this metal. nor by hydro-fulph. of amm.

(C 6.) The carbonates of potafh, of foda, or of ammonia, occafioned no precipitation; but the pure alkalis precipitated a yellow oxide, foluble by excefs of alkali, and alfo foluble in every acid that I have tried. nor by carb. alkalis; but by pure alk. foluble in acids.

(D 1.) The folution of this oxide in muriatic acid, upon being evaporated, did not cryftallize; the refiduum was foluble in alcohol, and of a rofe colour. Sal ammoniac, nitre, or common falt, caufed no precipitation from the muriatic folution; but formed triple falts, which were not foluble in alcohol. Muriatic folution of rhodium.

(D 2.) The folution in nitric acid alfo did not cryftallize. A drop of this folution, being placed upon pure filver, occafioned no ftain. On the furface of mercury a metallic film was precipitated, but did not appear to amalgamate. The metal was alfo precipitated by copper and other metals, as might be prefumed, from the ufual order of their affinities for acids. Nitric folution.

(E 1.) The precipitate obtained by zinc (C 2.) from the remaining half of the falt, appeared in the form of a black powder, weighing, when thoroughly dried, nearly 2 grains, correfponding to about 4 grains in the 1000 of ore diffolved. The precip. before made by zinc was black,

(E 2.)

fimply heated,

(E 2.) When expofed to heat, this powder continued black; with borax, it acquired a white metallic luftre, but appeared infufible by any degree of heat.

fufible with arfenic and with fulphur. Metallic button not malleable,

(E 3.) With arfenic, however, it is, like platina, rendered fufible; and, like palladium, it may alfo be fufed by means of fulphur. The arfenic, or the fulphur, may be expelled from it by a continuance of the heat; but the metallic button obtained does not become malleable, as either of the preceding metals would be rendered by fimilar treatment.

unites with metals except mercury.

(E 4.) It unites readily with all metals that have been tried, excepting mercury; and, with gold or filver it forms very malleable alloys, that are not oxidated by a high degree of heat, but become incrufted with a black oxide, when very flowly cooled.

Gold and rhodium 4 to 1.

(E 5.) When 4 parts of gold are united with one of rhodium, although the alloy may affume a rounded form under the blowpipe, yet it feems to be more in the ftate of an amalgam than in complete fufion.

Gold and rhodium 6 to one.

It does not difcolour gold.

(E 6.) When fix parts of gold are alloyed with one of rhodium, the compound may be perfectly fufed, but requires far more heat than fine gold. There is no circumftance in which rhodium differs more from platina, than in the colour of this alloy, which might be taken for fine gold, by any one who is not very much accuftomed to difcriminate the different qualities of gold. On the contrary, the colour of an alloy containing the fame proportion of platina, differs but little from that of platina. This was originally obferved by Dr. Lewis. "The "colour was ftill fo dull and pale, that the compound (five "to one) could fcarcely be judged by the eye to contain any gold."*

I find that palladium refembles platina, in this property of deftroying the colour of a large quantity of gold. When one part of palladium is united to fix of gold, the alloy is nearly white.

Solubility of rhodium affected by combination.

(E 7.) When I endeavoured to diffolve an alloy of filver or of gold with rhodium, the rhodium remained untouched by either nitric or nitro-muriatic acids; and, when rhodium had been fufed with arfenic or with fulphur, or when merely heated by itfelf, it was reduced to the fame ftate of infolubility. But,

* Lewis's Philofophical Commerce of Arts, p. 526.

when

when one part of rhodium had been fufed with three parts of bifmuth, of copper, or of lead, each of thefe alloys could be diffolved completely, in a mixture of two parts, by meafure, of muriatic acid, with one of nitric. With the two former metals, the proportion of the acids to each other feemed not to be of fo much confequence as with lead; but the lead appeared on another account preferable, as it was moft eafily feparated, when reduced to an infoluble muriate by evaporation. The muriate of rhodium had then the fame colour and properties, as when formed from the yellow oxide precipitated from the original falt. (D 1.)

(E 8.) The fpecific gravity of rhodium, as far as could be ascertained by trial on fo fmall quantities, feemed to exceed 11. That of an alloy confifting of one part rhodium and about two parts lead, was 11,3; which is fo nearly that of lead itfelf, that each part of this compound may be confidered as having about the fame fpecific gravity. Specific gravity.

F. As it was expected that the alcohol employed for wafhing the falt of rhodium (B 6.) would contain the foda-muriates of platina and of palladium, the platina was firft precipitated by fal ammoniac. This precipitate was of a deep red colour; and, when it had been heated, to expel the fal ammoniac, the platina which remained was of a dark gray colour. The alcohol before ufed in wafhing the falts examined.

(G 1.) To the remaining folution, after it had been diluted to prevent any further precipitation of platina, I added pruffiate of potafh, which inftantly occafioned a very copious precipitate, of a deep orange-colour at firft, but changing afterwards to a dirty bottle-green, which I afcribed to iron contained in the pruffiate.

(G 2.) This precipitate, when dry, weighed $12\frac{1}{2}$ grains. After it had been heated, it left a metallic refiduum, in fmall grains, of a gray colour, weighing nearly 7 grains. A fmall portion of it being heated with borax, communicated a dark brown colour to the borax, as from iron, and acquired a bright metallic luftre, but could not be fufed under the blowpipe. With fulphur, however, it fufed immediately into a round globule, which, by floating upon mercury, appeared of lefs fpecific gravity than that metal.

(G 3.) The whole quantity was then treated in the fame manner, and purified by cupellation with borax, till it cooled with a bright furface. From the globule the fulphur was expelled,

pelled, by expofure to the extremity of the flame; and it became fpongy and malleable, weighing in this ftate very nearly five grains.

Palladium.

(G 4.) A portion of this metal was diffolved in ftrong nitrous acid, was precipitated by green fulphate of iron, and in other refpects, poffeffed all the properties afcribed to the palladium offered for fale, in the printed paper that accompanied it, as well as others fince noticed by Mr. Chenevix.

Its differences from platina.

(G 5.) In its precipitation by pruffiate, it differs moft effentially from platina; and confequently is by no means difficult to be diftinguifhed, or feparated from it.

(G. 6.) The action of muriate of tin upon the folutions of thefe metals, is alfo totally different. A dilute folution of platina, is thereby changed from a pale yellow to a tranfparent blood-red. A folution of palladium, on the contrary, ufually becomes opaque, by the formation of a brown or black precipitate; but, if mixed in fuch proportion as to remain tranfparent, it changes to a beautiful emerald-green.

(G 7.) In the formation of triple falts with the alkalis, as obferved by Mr. Chenevix, palladium may be faid to refemble platina; but the falts thus formed are far more foluble than the correfponding falts of platina, and differ entirely, in the colour and form of the cryftals.

(G 8.) The foda-muriate of palladium is a deliquefcent falt; that of platina, on the contrary, forms permanent cryftals.

(G 9.) The triple falts of platina, with either muriate of ammonia or of potafh, form octaedral cryftals of a yellow colour, that are very fparingly foluble in water. The correfponding falts of palladium, likewife refemble each other in every refpect. The cryftals are very foluble in water, but infoluble in alcohol; their form is that of a four-fided prifm, and they each prefent a curious contraft of colour, that certainly is not obfervable in any known falt of platina.

Curious contraft of colour in the falts of palladium.

(G 10.) Although the folution is of a deep red, the cryftals are of a bright green when viewed tranfverfely. In the direction of their axes, however, the colour is the fame as that o the folution; but, on account of its extreme intenfity, it is with difficulty diftinguifhed in fragments that exceed $\frac{1}{100}$ of an inch in thicknefs. One confequence of this colour is neverthelefs

very

very obſervable; namely, that in viewing any cryſtal obliquely, it appears of a dull brown, that ariſes from a mixture of the red and green.*

Probability that Mr. Chenevix was miſled to infer that palladium is a compound, from its being found in platina.

The characters of palladium that have been enumerated, undoubtedly belong to none of the ſimple ſubſtances that we are acquainted with; and no experiment that I have made, has tended to confirm the ſuſpicion of its being a compound, conſiſting of any known ingredients. The experiments above related, ſhow evidently, that the ore of platina contains a very ſmall quantity of palladium; and it is not unlikely that this may have been a conſtituent part of ſome of the compounds obtained by Mr. Chenevix, and may have miſled him, by ſome properties which he would conſequently obſerve, into the ſuppoſition that he had formed palladium.

All attempts of the author to compare it were unſucceſsful.

It is not, however, without having repeatedly endeavoured to imitate his experiments, that I have ventured to diſſent from ſuch authority. I made many attempts to unite pure platina with mercury, by ſolution, and by amalgamation; but without ſucceſs, in any one inſtance.

From a ſolution of platina, carefully neutralized, as Mr. Chenevix directs, with red oxide of mercury, and mixed with a ſolution of green ſulphate of iron, I indeed obtained ſuch a

* The change of colour above deſcribed, though certainly uncommon, is nevertheleſs not peculiar to the ſalts of palladium, but may be ſeen alſo in ſome kinds of tourmalin. Among thoſe which come to us from Ceylon, ſome are tranſparent; and one variety is of a deep red in the direction of its axis, but of a yellowiſh green when viewed tranſverſely. There is alſo a correſponding, but oppoſite contraſt of colours, that has been obſerved by Muller, and deſcribed by Bergmann, in ſome of the Tyroleſe tourmalins. The general aſpect of theſe ſtones is black, and apparently opaque.—Some, however, of which the fracture is vitreous, are found to tranſmit a yellowiſh-red light when viewed tranſverſely, but in the direction of their axis the colour is a dull bottle green.

In each of theſe tourmalins, as well as in the ſalts of palladium, the colour in the direction of the axis is at leaſt ten times more intenſe than in the tranſverſe direction. A thin lamina, cut from the end of a Tyroleſe tourmalin for this purpoſe, tranſmitted no viſible light, till it was reduced to $\frac{1}{60}$ of an inch in thickneſs; and, when leſs than $\frac{1}{100}$ of an inch, it was not more tranſparent than another portion of the ſame cryſtal ſeen tranſverſely, $\frac{1}{10}$ of an inch in thickneſs.

precipitate

precipitate of metallic flakes as he defcribes; but, upon examination of thefe flakes, they yielded mercury by diftillation; and the remainder confifted of platina combined with a portion of iron, but had not any properties which I could fuppofe owing to the prefence of palladium.

Other facts and obfervations to fhow that palladium is a fimple metal.

Upon comparing the fpecific gravity of this fubftance, which was faid to be, at moft, 11,8, with that of mercury or of platina, I was always ftrongly inclined to doubt the poffibility of its being compofed of thefe metals. I could recollect no one inftance, in which the fpecific gravity of any compound is lefs than that of its lighteft ingredient, and could not, without careful examination, admit the fuppofition, that mercury could be rendered lighter by intimate union with platina. It now appears fully confirmed that this perfuafion, arifing from uniform experience, was well founded; for, if we confider the difficulty of producing even an imperfect imitation of palladium, the failure of all attempts to refolve it into any known metals, the facility of feparating it from any mixed folution of thofe which it has been fuppofed to contain, as well as the number and diftinctnefs of its characteriftic properties, I think we muft clafs it with thofe bodies which we have moft reafon to confider as fimple metals.

XI.

Letter from Mr. WM. WILSON, *exhibiting the Electricity of Metals, without the Help of any condenfing Inftrument.*

To Mr. NICHOLSON.

SIR,

London, Dec. 22, 1804.

Whether the electricity from metals alone be the effect of the contact or the feparation,

WHEN I fet about making the compound electrical condenfer defcribed in my laft letter to you, I intended to repeat the experiments of Cavallo relating to the electricity obtained by the contact of metals related in the third volume of the fourth edition of his Treatife of Electricity; but before the inftrument was finifhed, I was induced (by fome experiments I had made relative to the caufe of excitation of electricity) to fuppofe that it is not the contact of the metals that is the caufe of the appearance of electrical figns, but the feparation of the metals

metals from contact. And this ſuppoſition was very much ſtrengthened, when, upon examination, I found that all who have made experiments on this ſubject have ſeparated the metals from contact before they examined them as to their electricity.

—determinable by varying the ſurfaces.

If the contact of the two metals be the cauſe of the electrical ſigns, the whole effect that one metal can have on another will be communicated at the time of contact, however few the points are that form the contact, becauſe both the metals being conductors of electricity, if one poſſeſſed a greater proportion of it than the other, a part will be communicated to the other at the time of contact, to form an equilibrium. and this will be done as well by a few points of contact as by a great many. But if it is the ſeparating them from contact, that is the cauſe of the electrical ſign, the more extenſive the contact is the more powerful will the ſigns be when the metals are ſeparated.

Sieve of one metal and filings of another.

To put this to the teſt of experiment, I pierced a piece of thin ſheet copper full of ſmall holes, juſt big enough to permit to paſs through them two or three particles of filings of another metal at a time, ſo that almoſt every particle muſt be in contact with the copper before it can paſs through, and conſequently the ſurface of contact be very great with a comparatively ſmall quantity of metal.

I ſifted through this copper ſieve ſome filings of zinc into a tin plate laid on the cap of a gold leaf electrometer, and the gold leaves diverged near an inch with poſitive electricity, when about half an ounce of filings had been ſifted into it. Encouraged by this ſtriking reſult, I procured ſieves and filings of different metals. The reſults of the trials with them are contained in the following table; where P ſtands for poſitive electricity, N for negative, and when it was not ſtrong enough to effect the electrometer, that is denoted by O.

Reſults of experiments.

Filings of	Sifted through	Effect.
Copper	Copper	O.
	Tin	N. ſtrong
	Silver	O.
	Zinc	P. ſtrong
	Lead	N.
Zinc	Copper	P. ſtrong
	Tin	O.
	Silver	P. ſtrong
	Zinc	P. exceeding weak
	Lead	O.

Refults of experiments.

Filings of	Sifted through	Effect.
Tin	Copper	P. rather ftrong
	Tin	N. very weak
	Silver	P. ftrong
	Zinc	O.
	Lead	N. weak
Silver	Copper	O.
	Tin	N. weak
	Silver	O.
	Zinc	N.
Steel	Copper	N. very ftrong
	Tin	N. ditto
	Silver	N. ditto
	Zinc	N. ditto
	Lead	N. ditto
Lead	Copper	P. very ftrong
	Tin	O.
	Silver	P. very ftrong
	Zinc	P. not quite fo ftrong as with filver
	Lead	O.
Brafs	Copper	P. very ftrong
	Tin	N. ftrong
	Silver	P. ftrong
	Zinc	O.
	Lead	N.
Bifmuth	Copper	P. exceeding ftrong
	Tin	O.
	Silver	P. exceeding ftrong
	Zinc	P. ftrong
	Lead	N.
Antimony	Copper	O.
	Tin	N. very ftrong
	Silver	P. ftrong
	Zinc	N. ftrong
	Lead	N. very ftrong
Nikel	Copper	P. exceeding ftrong
	Tin	O.
	Silver	P. ftrong
	Zinc	O.
	Lead	P.

In

In all the above experiments there was a large ſurface of contact, and the electrometer only was uſed; but in thoſe made by Cavallo and others who had a very ſmall ſurface of contact, electrical ſigns could not be made to appear without the help of doublers, multipliers, &c. of electricity. I therefore think there can be no doubt about the ſeparating the metals from contact being the cauſe of their appearing.

If you think the above worthy of a place in your Phlioſophical Journal, you will very much oblige me by inſerting it therein. I am your obedient humble ſervant,

WM. WILSON.

XII.

Analytical Experiments and Obſervations on Lac. By CHARLES HATCHETT, *Eſq. Partly abridged, but chiefly extracted from the Philoſophical Tranſactions for* 1804, *by* W. N.

IT is uncertain at what time the uſeful ſubſtance, called Lac, firſt became known in Europe. It has long been uſed by the Indians for purpoſes different from thoſe to which it is applied in Europe. In particular as Mr. Wilkins informed the author, they diſſolve ſhell lac in water by the addition of a little borax, and by adding lamp black or ivory black, they obtain an ink, not eaſily affected by water, when dry.

Uſes of lac peculiar to India.

Aqueous ſolution by borax affords an ink.

The natural hiſtory of lac, which is the nidus of the coccus or chermes lacca, has been detailed by Mr. Kerr, Mr. Saunders, and Dr. Roxburgh, in the Philoſophical Tranſactions for 1781, 1789, and 1791. It is diſtinguiſhed into four kinds, of which, however, only three are commonly known in commerce, *viz.* ſtick lac, ſeed lac, and ſhell lac; the difference of theſe, with that of the fourth, called lump lac, is as follows.

Natural hiſtory of lac.

Four kinds.

1. Stick lac, is the ſubſtance or comb in its natural ſtate, incruſting ſmall branches or twigs.

1. Stick lac.

2. Seed lac, is ſaid to be only the above, which has been ſeparated from the twigs, and reduced into ſmall fragments; but I ſuſpect it to have undergone ſome other proceſs, as I have found the beſt ſpecimens to be very conſiderably deprived of the colouring matter.*

2. Seed lac.

3. Lump

* Mr. Wilkins informs me that the crude lac, as it is taken from the branches and twigs of the trees, is uſually deprived of its colouring

3. Lump lac.

3. Lump lac, is formed from feed lac, liquefied by fire, and formed into cakes. And,

4. Shell lac.

4. Shell lac, according to Mr. Kerr and Mr. Saunders, is prepared from the cells, liquefied, ftrained, and formed into thin tranfparent laminæ, in the following manner.

How made from the cells.

" Separate the cells from the branches; break them into fmall " pieces; throw them into a tub of water, for one day; wafh " off the red water; dry the cells, and with them fill a cylindri- " cal tube of cotton cloth, two feet long, and one or two inches " in diameter; tie both ends, and turn the bag above a charcoal " fire; as the lac liquefies, twift the bag, and, when a fufficient " quantity has tranfuded the pores of the cloth, lay it upon a " fmooth junk of the plantain tree, and with a ftrip of the plan- " tain leaf draw it into a thin lamella; take it off while flexible, " for in a minute it will be hard and brittle." †

The degree of preffure on the plantain tree, regulates (according to Mr. Saunders) the thicknefs of the fhell; and the quality of the bag determines its finenefs and tranfparency.

Lac is moftly had from Affam.

Affam furnifhes the greateft quantity of the whole of the lac now in ufe. ‡

Beft quality.

Mr. Kerr (fpeaking of ftick lac) obferves, that the beft lac is of a deep red colour; for, if it is pale and pierced at the top, the value is diminifhed, becaufe the infects have left their cells, and confequently thefe can be of no ufe as a dye or colour, but probably may be better for varnifhes.

The feed lac which I have examined, contained but little of the colouring matter, and appeared (as I have already obferved) to have undergone fome procefs of purification; but, of all the varieties, fhell lac contains the leaft of the tinging fubftance, as may well be expected, when the mode of preparing it is confidered.

Chemifts have paid little attention to lac.

It is remarkable, that although lac has been known, and imported into Europe, during fo long a time that the date cannot now be afcertained, yet it has but little attracted the attention of chemifts.

ing matter by boiling, having been previoufly reduced, by pounding into fmall fragments. In Bengal, the filk dyers are the people who thus produce what we call the feed lac, which they do for the fake of the colour.

† Phil. Trans. 1781, p. 378. ‡ Phil. Tranf. 1789, p. 109.

The firſt chemiſt of eminence who mentions it, and the only one who has ſubjected it to any thing like a regular examination, is the younger Geoffroy, whoſe Paper is publiſhed in the *Mém. de l'Acad. de Paris* for the year 1714.* In this Paper, Mr. Geoffroy ſeems to have been chiefly induced to examine it on account of its tinging ſubſtance; but he nevertheleſs has not neglected the ſubſtance which conſtitutes the cells. This he conſiders to be a ſort of wax, very diſtinct from the nature of gum or reſin. But it is to be obſerved, that he formed this opinion, not ſo much upon the reſults of chemical experiments, as upon the cellular conſtruction obſerved in the ſtick lac, which, as he juſtly remarks, demonſtrates it to be formed by inſects, after the manner that the honeycomb is formed by bees; and that it is not therefore, as ſome have ſuppoſed, a gum or reſin, which has exuded from vegetables ſimply punctured by inſects.† Experiments of Geoffroy the younger;

Geoffroy and Lemery obtained from lac, by diſtillation, ſome acid liquor, and a butyraceous ſubſtance. Moreover, Geoffroy obſerves, that when ſtick lac was thus treated, ſome ammonia was alſo obtained, but not when ſeed lac was employed. —and Lemery.

He alſo mentions another ſort of lac, brought from Madagaſcar, and called by the natives *Lit-in-bitſic.* This ſubſtance, he ſays, is ſcarcely to be diſtinguiſhed from bees-wax, which it much reſembles in colour and odour; and that it is produced by a grayiſh inſect, much larger than the chermes lacca. It is evident however, from Geoffroy's deſcription, that this ſubſtance is very different from the common lac; and there can be little doubt, but that it is the ſame as that which was, a few years ago, examined by Dr. Pearſon, under the name of white lac, a ſubſtance reſembling the Pé-la of the Chineſe. ‡ Another lac which appears to have been the Pe-la of China.

Geoffroy (as I have ſtated) conſidered lac as a ſort of wax; and ſince his time it has ſcarcely, if at all, been ſubjected to Notions of chemiſts concerning lac.

* Obſervations ſur la Gomme Lacque, et ſur les autres Matières animales qui fourniſſent la Teinture de Pourpre. Par. M. Geoffroy le jeune. *Mém. de l'Acad.* 1714. p 121.

† Mr. Kerr obſerves, that as a red ſubſtance is obtained by inciſion from the plaſo tree, very analogous to lac, it is probable, that the inſects have little trouble in animalizing the ſap of theſe trees, in the formation of their cells. Phil. Trans. 1781, p. 377.

‡ Phil. Trans. 1794, p. 383.

chemical

chemical examination; it is not therefore furprifing, that the opinions of chemifts concerning it have been various. Chaptal adopts the opinion of Geoffroy, and calls it a kind of wax; * but Gren † and Fourcroy ‡ regard it as a true refin.

§ I.

Chemical Experiments on Lac. Its Habitudes with Solvents.

Water extracts the colour from lac.

1. Water digefted upon powdered ftick lac becomes of a deep crimfon colour, and takes up about 10 per cent. Seed lac gives out no more than $2\frac{1}{2}$ or 3 per cent. and fhell lac only $\frac{1}{2}$ per cent. by fimilar treatment.

Alcohol diffolves the refin, which is a large part.

2. Alcohol diffolves much of the lac, taking up refin in a clear folution if cold; but if heated, the folution is turbid and fcarcely to be rendered tranfparent, either by repofe or filtration.

The refin is obtained either by evaporating the folution or by pouring it into water acidulated with muriatic or acetic acid. The refin which is thus precipitated may be feparated by the filter. It amounts to 67 or 68 per cent. in ftick lac, and to 88 in the feed lac. Shell lac contains 81 per cent. befides 10 per cent. which is defended from the fpirit, and cannot be feparated but by fubfequent operation. The refin is lefs brittle than other refin.

Vegetable gluten.

When the fhell lac was in fmall fragments only, thefe after the firft feparation of the refin, retained their figure and were more bulky, porous, and elaftic. The elafticity was deftroyed by boiling water, and the matter itfelf proved to be of the nature of vegetable gluten.

Ether affects lac very little.

3. Sulphuric ether acts lefs powerfully on lac than alcohol does. It confiderably foftens the lac; but produces little other effect.

Sulphuric acid converts it into coal.

4. Concentrated fulphuric acid firft acts on the colouring matter; but after a fhort digeftion on the fand bath, the fluid acts on the whole mafs, and becomes firft reddifh brown, and afterwards black, fulphureous acid gas being evolved, and the chief part of the lac being at length feparated in a ftate refembling coal.

* Chaptal's Elements; Englifh edition. Vol. III. p. 387.

† Principles of modern Chemiftry. Vol. I. p. 388.

‡ *Syftême des Connoiffances chimiques*, Tome V. p. 624.

Nitric

5. Nitric acid applied to lac emits nitrous gas at firſt, and cauſes the lac to ſwell much at the ſame time that it converts it into a deep opake yellow brittle ſubſtance, which by a ſufficiency of nitric acid, and continuance of the digeſtion for 48 hours, is diſſolved. The ſolution however is turbid, and when poured into a large quantity of diſtilled water, depoſits ſome yellowiſh flocculi, which, being collected, are found to be a ſort of wax.

Nitric acid by management and time diſſolves it.

The ſolution is turbid and by dilute depoſits a wax.

The filtrated liquor is of a bright golden yellow; and, when ſaturated by ammonia, changes to orange colour, but does not yield any precipitate, nor any traces of oxalic or malic acid.

The filtered ſolution contains a yellow matter.

This yellow nitric ſolution is converted, by evaporation, into a deep yellow ſubſtance, which burns like reſin, but is ſoluble in boiling water.

Its properties, &c.

The alkalis and lime, being added to this aqueous ſolution, do not produce any precipitate, but the yellow colour is very conſiderably deepened; and, by evaporation, an orange-coloured ſubſtance is obtained, which is ſtill eaſily ſoluble in water, and conſiſts of the deep yellow ſubſtance above-mentioned, combined with the alkali or lime.

6. Muriatic acid diſſolves the colouring matter and gluten of lac; but its action on theſe is feeble, unleſs the reſin has been previouſly ſeparated.

6. Muriatic acid diſſolves the colouring matter and gluten.

7. Acetous acid, in its effects, much reſembles muriatic acid.

7. Acetous acid.

8. Stick lac, ſeed lac, and ſhell lac, are partially diſſolved by acetic acid; and, if this be heated, a conſiderable portion is taken up.

8. Acetic acid acts upon the whole except the wax.

The diſſolved part conſiſts of the colouring extract, of reſin, and of gluten; the wax being the only ingredient which is inſoluble in this menſtruum; but a portion of the former ſubſtances, being enveloped by the wax, are protected from the action of the acetic acid.

The acetic ſolution of lac becomes turbid when cold, and depoſits part of the reſin; a portion however remains in ſolution, and may be precipitated by water; after which, the liquor retains ſome gluten and colouring extract, which may be precipitated by ſaturating the acid with an alkali, and by ſubſequent boiling.

For the reaſons above ſtated, it would be difficult to effect a complete ſolution of lac by means of acetic acid; but this may

 neverthele∫s

neverthelefs be advantageoufly employed in analytical operations, when alternately ufed with alcohol.

Boracic acid fcarcely acts on lac.

9. A faturated folution of boracic acid in water, diffolves the colouring extract; but, as the effect does not furpafs that of water alone, we may conclude that lac is little, if at all, acted upon by boracic acid.

Borax renders it foluble in water.

10. It has been already ftated, that fub-borate of foda or borax has a powerful effect on lac, fo as to render it foluble in water; and, as the preceding experiments prove that boracic acid alone fcarcely acts upon lac, there is every reafon to believe, that the excefs of foda prefent in borax is the active fubftance; and this conclufion will be confirmed, by the refults of fubfequent experiments made with the alkalis.

Proportion, &c.

In order to render lac (efpecially fhell lac) foluble in water, about $\frac{1}{5}$ of borax is neceffary; and this may be previoufly diffolved in the water, or may be mixed and added together with the lac.

The beft proportion of water to that of lac is 18 or 20 to 1. So that 20 grs. of borax, and four ounces of water, are, upon an average, requifite to diffolve 100 grs. of fhell lac; but more water may be occafionally added, to fupply the lofs caufed by evaporation during the digeftion, which fhould be made nearly in a boiling heat.

This folution of fhell lac is turbid, and of a reddifh-brown colour; when confiderably diluted with water and agitated, a weak lather is formed; it is decompofed by acids, and the lac is precipitated in yellow flocculi, which do not apparently differ from the lac originally employed.

The folution is a foap of difficult folubility.

The general properties of the folution fhew, that it is a faponaceous compound, which, being ufed as a varnifh or vehicle for colours, becomes (when dry) difficultly foluble in water, although this was the liquid employed to form the folution.

A white thick fcum or cream collects on the furface of this liquid, after it has been fuffered to remain tranquil for fome time, and is found to be produced by a fort of wax, which I fhall more particularly notice when the analyfes of the varieties of lac are defcribed; but, in the prefent cafe, this wax appeared in fome degree to be converted into an almoft infoluble foap by the alkali of the borax, and may be regarded as the principal caufe of the turbidnefs of the folution.

11. The

11. The lixivia of pure ſoda and of carbonate of ſoda completely diſſolve the different kinds of lac; and theſe ſolutions exactly reſemble thoſe formed by means of borax, excepting that they are deeper coloured. Soda and its carbonate diſſolve lac.

Rather leſs than $\frac{1}{5}$ of carbonate of ſoda is required to diſſolve ſhell lac; and this ſolution, when dried, is ſooner affected by damp or water than the ſolution prepared by borax.

12. Lixivium of pure or cauſtic potaſh ſpeedily diſſolves the varieties of lac, and forms ſaponaceous ſolutions, ſimilar to that in which borax was employed, excluſive of the colour, which is deeper, and more approaching to purple. Alſo potaſh,

Lixivium of carbonate of potaſh extracts a great part of the colouring matter, but does not form ſo complete a ſolution of the entire ſubſtance of lac, as when pure potaſh is employed. and its carbonate.

The above alkaline ſolutions, by repoſe, afford the waxen ſoap which has been mentioned; and acids, being added to theſe ſolutions, and to that formed by borax, precipitate the lac in a flocculent ſtate, and of a yellow or buff colour, which precipitate, when melted, becomes ſimilar to the lac originally employed. If however an alkaline ſolution of ſhell lac (prepared, for inſtance, with ſoda) be gradually dropped into a ſufficient quantity of muriatic acid diluted with an equal portion of water, and nearly heated to the boiling point, and if after boiling the whole for about one hour the coagulum be ſeparated, and the clear liquor be carefully ſaturated with ſoda, and again made to boil, a ſmall quantity of a flocculent precipitate is obtained, which was found to be analogous to precipitated vegetable gluten, combined with ſome of the colouring extract.

13. Pure ammonia, and carbonate of ammonia, readily act upon the colouring matter of lac, but do not completely diſſolve the entire ſubſtance. Ammonia acts partially.

§ II.

Analytical Experiments on Stick, Seed, and Shell Lac.

Lac, placed on a red hot iron, contracts, melts, ſmokes much, and leaves a ſpongy coal. Slow diſtillation carried to the red heat, gave from ſtick lac, 1. Water ſlightly acid, 10; Products of lac by deſtructive diſtillation. Stick lac.

 2. Thick

2. Thick brown butyraceous oil, 59; 3. Spongy coal, 13½; 4. A ſmall portion of carbonate of ammonia, with carbonic acid, carbonated hidrogen, and hidrogen gas, by eſtimate 17½.

Seed lac.

Seed lac, by the ſame proceſs, gave, 1. Acidulated water, 6; 2. Thick oil, 61; 3. Spongy coal, 7; 4. Mixed gas as before, but without ammonia, 26.

Shell lac.

Shell lac gave, 1. Acidulated water, 6; 2. Oil, 65; 3. Spongy coal, 7½; 4. Mixed gas, 21½.

The coal of this laſt gave one grain of aſhes after combuſtion, which contained a muriate, probably of ſoda. There was alſo a little iron, and ſome particles of ſand, probably extraneous.

Analyſis of Stick Lack.

Powdered ſtick lac gave a red colouring extract to water.

A. 200 grains of ſtick lac, picked and reduced to powder, were digeſted in a pint and a half of boiling diſtilled water during 12 hours. The liquor was tranſparent, and of a beautiful deep red; this was decanted into another veſſel; and the operation was repeated, with freſh portions of water, until it ceaſed to be tinged; the lac then appeared of a pale yellowiſh-brown colour.

The whole of the aqueous ſolution being evaporated, left a deep red ſubſtance, which poſſeſſed the general properties of vegetable extract, and weighed 18 grains.

Alcohol then took up much reſin.

B. The dried lac was digeſted for 48 hours, without heat, in eighteen ounces of alcohol; and the clear tincture being cautiouſly decanted different portions of alcohol were added, and the digeſtion was repeated, until the alcohol ceaſed to produce any effect.

The whole of the ſolutions in alcohol were then poured into diſtilled water, which was heated, and an attempt was made to ſeparate the precipitated ſubſtance by filtration; but, as this did not ſucceed, on account of the filter ſpeedily becoming clogged, the whole was ſubjected to gentle diſtillation; by which, a browniſh-yellow reſin was obtained, amounting in weight to 136 grains.

C. The remainder of the lac was again digeſted in boiling diſtilled water; by which, 2 grains of the colouring extract were obtained.

Diluted muriatic acid took gluten from the remainder.

D. The reſiduum was then digeſted with one ounce of muriatic acid diluted with two ounces of water, which, by boiling,

ing, became of a bright pale red, but changed to purple, when ſaturated with a ſolution of carbonate of potaſh.

A flocculent precipitate was thus obtained, which poſſeſſed the characters of precipitated vegetable gluten combined with ſome of the colouring extract; this, when completely dried, weighed 11 grains.

E. There now remained 25 grains, which evidently conſiſted of a ſort of wax, mixed with ſmall parts of twigs and other extraneous ſubſtances. The reſidue was wax and foreign matter.

A part of the wax was ſeparated by heat and preſſure in a piece of linen; and another portion was ſeparated by digeſtion in olive oil, which aſſumed the conſiſtency of an unguent.

The reſiduum was then boiled with lixivium of potaſh, and became tinged with purple, in conſequence of ſome of the colouring extract which had not been diſſolved by the preceding operations.

The undiſſolved part, now conſiſting only of the extraneous vegetable and other ſubſtances, weighed 13 grains; ſo that the wax, with a ſmall portion of the colouring extract, may be eſtimated at 12 grains.

By the above proceſs, 200 grains of ſtick lac afforded, Component parts of ſtick lac.

			Grs.
A. C.	Colouring extract	18 2	20
B.	Reſin		136
D.	Vegetable gluten		11
E.	Wax, with a little colouring extract, about		12
	Extraneous ſubſtances		13
			192.

Analyſis of Seed Lac.

200 grains of very pure ſeed lac were ſubjected to operations very ſimilar to thoſe which have been deſcribed, and afforded, Component parts of ſeed lac.

	Grs.
Colouring extract	5
Reſin	177
Vegetable gluten	4
Wax	9
	195.

Analyſis

Analysis of Shell Lac.

Shell lac boiled in water gave little extract.

A. 500 grains of this substance were first treated with boiling distilled water, as above-mentioned, and yielded of extract only 2.50 grains.

Residue gave much resin to alcohol.

B. The 497.50 grains which remained, were then digested with different portions of cold alcohol, until this ceased to produce any effect; the resin which was thus separated, amounted to 403.50 grains.

C. As the shell lac had not been reduced into powder, but only into small fragments, these were become white and elastic, and, when dry, were brittle, and of a pale brown colour; the whole then weighed 94 grains.

Muriatic acid then took up gluten.

D. These 94 grains were digested in diluted muriatic acid; and the acid, being afterwards saturated with solution of carbonate of potash, afforded a flocculent precipitate, (resembling that obtained from solutions of vegetable gluten,) which, when dry, weighed 5 grains.

Acetic acid, by long digestion, took up resin and wax; the latter was thrown down by alcohol;

E. Alcohol acted but feebly on the residuum; it was therefore put into a matrass, with three ounces of acetic acid, and was suffered to digest without heat during six days, the vessel being at times gently shaken; the acid thus assumed a pale brown colour, and was very turbid. The whole was then added to half a pint of alcohol, and was digested in a sand-bath; by which a brownish tincture was formed, and at the same time a quantity of a whitish flocculent substance was deposited, which, being collected, well washed with alcohol on a filter, and dried, weighed 20 grains.

This substance was white, light, and flaky, and, when rubbed by the nail, it became glossy, like wax; it also easily melted, was absorbed by heated paper, and, when placed on a coal or hot iron, emitted a smoke, the odour of which very much resembled that of wax, or rather spermaceti.

and then the resin by water.

F. The solution formed by acetic acid and alcohol, being filtrated, was poured into distilled water, which immediately became milky; and, being heated, the greater part of the resin which had been dissolved assumed a curdy form, and was partly separated by a filter, and partly by distilling off the liquor; this portion of resin amounted to 51 grains.

Carbonate of potash threw down some resin.

G. The filtrated liquor, from which this resin had been separated, was saturated with a solution of carbonate of potash;

potafh; and, being heated, a fecond precipitate of gluten was obtained, which, when well dried, weighed 9 grains.

The 500 grains of fhell lac thus yielded,

Component parts of fhell lac.

		Grs.
A.	Extract	2.50
B. C.	Refin	454.50
D. G.	Vegetable gluten	14.
E.	Wax	20.
		491.

Advantages of the procefs laft ufed.

The mode of analyfis adopted for the fhell lac, muft undoubtedly appear lefs fimple than that which was employed for feed and ftick lac: but, upon the whole, it was attended with advantages; for the fhell lac being in fmall fragments, and not in the ftate of a powder, confiderably facilitated the decantation of the folution in alcohol from the refiduum; and although, in this laft, a portion of the refin was protected from the action of the alcohol, by being enveloped in the gluten and wax, yet, by the affiftance of acetic acid, the remainder of the refin, as well as the whole of the gluten, were diffolved; the wax was obtained in a pure ftate; and a feparation of the refin from the gluten was afterwards eafily effected, by the method which has been defcribed. As therefore acetic acid is capable of diffolving refin, gluten, and many other of the vegetable principles, it certainly may be regarded as a very ufeful folvent, in the analyfis of bodies appertaining to the vegetable kingdom.

From the refults of the preceding analyfes it appears, that the different kinds of lac confift of four fubftances, namely, extract, refin, gluten, and wax, the feparate properties of which fhall now be more fully confidered.

(The Conclufion in our next.)

Galvanic

XIII.

Galvanic Illuſtrations and Remarks. By C. WILKINSON, *Eſq.*

To Mr. NICHOLSON.

DEAR SIR, *Glaſgow, Dec.* 9, 1804.

On the galvanic charge, &c.

HAVING, during my galvanic tour, been occaſionally informed that the attempt I have made in my Elements of Galvaniſm, to explain the laws of galvaniſm on the more perfect and imperfect conductors, is not ſo clear as it was my wiſh to have rendered it; I ſhall therefore deem myſelf much obliged by the inſertion of the following obſervations in your valuable Journal.

The power of a galvanic apparatus, formed of plates not leſs than $2\frac{1}{4}$ inches in diameter, on animal ſubſtances, is directly as the number of plates; on the more perfect conductors, as metals, the power is as you have at large ſtated in your laſt Journal.

Let us ſuppoſe three galvanic plates: *Fig.* 1, *(Plate II.)* of four inches; *Fig.* 2, of ſix inches; and *Fig.* 3, of eight inches: An arrangement of theſe in the form of a galvanic trough, produces equal effects on animal ſubſtances, provided the number be equal.

Animal ſubſtance is very little ſuperior to water in its conducting power; and the experiments of Mr. Cavendiſh have proved, that the conducting powers of iron are a million times greater than the conducting powers of water.

A metal brought into contact with another metal diſpoſed to part with a quantity of its compound electricity, will divide the quantity thus diſengaged, becauſe its capacity for receiving it is equal to that of the metal under the chemical action.

Animal ſubſtance brought into contact with a metal ſimilarly diſpoſed, will only receive a portion adequate to its capacity; and as this capacity is in the ratio of its conducting powers, it will be nearly one million times inferior in capacity to that of a metallic ſubſtance; and will, therefore, abſtract only ſo ſmall a proportionate part of electricity to render its intenſity equal to that exiſting in the galvanic plate.

B

By electrical intensity I understand the quantum of electricity multiplied by the resisting powers of the substance to admit it. Let us suppose the plates 1, 2, and 3, disposed to give out electricity on every assignable point of their respective surfaces. A finger applied at No. 1, *Fig.* 1, will abstract a certain portion of electricity from the points of metal directly in contact; a certain portion of electricity will immediately be determined from the squares 2, 3, 4, 5, 6, &c. from every corresponding square on the surface of the plate, by that motion which Volta has well termed the moto-electrical power: there is a physical resistance in the transmission from these respective squares to the first touched by the finger, and the more remote the greater the resistance: at whatever points this physical resistance shall be equal to the capacity of the substance employed, there all further transmission must cease: this resistance in plates of $2\frac{1}{4}$ inches in the square, appears, from experiment, fully adequate to the constant supply of so imperfect a conductor as animal substances, and therefore any increase of size in the surface of the plate, will produce no additional effects.

On the galvanic charge, &c.

When a certain quantity is thus abstracted from the pair of plates at the extremes of the battery, in direct contact with the fingers, these plates and the interposed fluid in the cells, only act as conductors to the next pair, and so on; as the velocity of discharges of each successive pair is so infinitely greater than the time required for the chemical action, is the reason why the effects we experience are increased in proportion to the numbers: if we were to suppose that the time required for the successive discharge of 50 pair of plates, should be precisely equal to the time required for the chemical action, we should derive no increase of power by an increase of number, which is contrary to experience.

I have a battery formed of 600 plates, each plate only exposing half a square inch in surface: To experience equal effects from this arrangement, I am obliged to pause three or four minutes between each contact; an interval of time no ways requisite in larger plates.

When such superior conductors as metals are employed, the physical resistance of transmission from one part of the plate to another, bearing no proportion with the capacity of the

the metal, every affignable point of the galvanic plate is efficient at the fame time, therefore the intenfity will be as the furface, and the powers as the fquares of the intenfities.

I am, Dear Sir,

Your's, refpectfully,

C. WILKINSON.

No. 19, *Soho Square.*

XIV.

On the Devitrification of Glafs and the Phenomena which happen during its Cryftallization. By DARTIGUES. *

Glafs is not a cryftallization.

SOME philofophers have confidered glafs as a chryftallization; and this opinion appears natural on confidering the tranfparence of glafs or of cryftal, from the latter of which the term for defignating the regular and fpontaneous arrangement of the particles of bodies has been taken. But on clofer reflection, this notion is found to be erroneous. In fact glafs never affects a cryftalline figure, either in its furface or its fracture; it never prefents chryftals of its own proper fubftance, fuch as are feen in metals flowly congealed; and whenever cryftals are formed in the mafs of glafs, they are foreign to the part which ftill continues vitrified; they may be confidered as a ftep the reverfe from that of vitrification, as I fhall fhow in the courfe of the prefent paper.

Vitreous fufion diftinct from that of falts.

On the firft explanation of vitreous fufion, I muft diftinguifh and feparate it from the fufion of certain other bodies which flow without addition in the heat of our furnaces, fuch as borax, the phofphoric acid and others. In thefe bodies the condenfed

* Tranflated from the Journal de Phyfique, LIX. 5.

The author of this memoir, who is proprietor of the glafs-works and other eftablifhments of Voneche (Sambre-et Meufe) has undertaken to draw up for the French National Inftitute a treatife on the art of glafs-making, to ferve as a continuation to the arts and trades of the Academy. The firft part is in readinefs to appear, many of the plates being ready. The fecond part relates to the ufes of glafs in the arts. And the third part confifts of detached memoirs upon the phyfical and chemical properties of glafs. The prefent differtation is extracted from one of thofe memoirs.—J. C. Delametherie.

denfed heat foftens and renders them fluid, and they more or lefs continue to exhibit, when cooled, the tranfparency and fome other of the well known phyfical properties of glafs.

But in the examination and defcription of that fufion to which vitreous compounds are fubjected in order to fit them for the purpofes of life, it is neceffary to obferve that the effect arifes from two phenomena. It is not fimply the refult of accumulated heat, but it is jointly produced by the affinities of the fubftances which enter into the mixture. Thefe fubftances which tend to combine and mutually to penetrate each other, act according to the laws of their affinities as foon as ever the temperature is fufficiently elevated. Thus it is that a mixture of feveral earths becomes fufed at a temperature which could not have rendered any one of them fluid. It is a folution by heat.

The common vitrification is therefore the refult of a combination effected at an elevated temperature between different and heterogenous fubftances; and the product is a compound perfectly homogenous more or lefs tranfparent, elaftic, and breaking in a peculiar manner, from which the term vitreous fracture is derived. This body is a remarkably bad conductor of heat and of electricity; it is capable of becoming foft at a temperature inferior to that at which it was fufed, fo that it may be rendered pafty, ductile, &c. Nature of the compound; or glafs.

The phenomenon during which all thefe properties difappear is what I have called devitrification; an expreffion which may at firft feem extraordinary, but which the facts will fhew to be perfectly accurate. Devitrification.

Several philofophers have before noticed this effect; fome have even made obfervations upon it, and have noted various circumftances fcarcely connected with each other; but I am not aware that any one has yet publifhed a feries of refearches proper to explain the effects, fo as to fhew that it is one of the known properties of all natural bodies, being in fact nothing more than the product of a cryftallization. It is the confequence of cryftallization.

Reaumur firft obferved that a glafs, more efpecially if it be compofed of different earths, as bottle glafs is in general, may be decompofed, and lofe its tranfparency and other vitreous properties. Fully occupied in his refearches upon porcelains, he was defirous of applying this difcovery to the fabrication of potteries, and attributed the phenomenon to the fubftances in which he had cemented his glafs. The fact was The porcelain of Reaumur:

 denomi-

denominated the cementation of glafs, and the product was called Reaumur's porcelain: no incident could be more effectually calculated to retard the true developement of the fact than fuch a denomination of this kind.

Repetitions of the procefs by d'Antic and others.

The labours of Bofc d'Antic upon the fame object were alfo directed to no other purpofe than that of obtaining a good pottery by this means, and of afcertaining what cements were the beft calculated to give new properties to this body. Thus it was that by calling the procefs by the name of cementation, which depended in no refpect upon what was added as the cement, others were mifled, who were induced to follow the courfe of experiments already began. The natural confequence was, that fcience gained nothing refpecting this procefs fince the time of Reaumur. Many have fince attempted the cementation of glafs without perceiving any new refult in the product.

Obfervation of cryftals in glafs.

Several perfons have fince admitted the property in glafs of affording cryftals; but thefe remarks being more efpecially made by artifts, placed by their fituation at the head of glafs-works, have not afforded the fcientific confequences which might have been deduced from them. The directors of a great eftablifhment have feldom time to dwell upon the contemplation of fmall effects; they are obliged to attend to too many things at the fame time. Thefe remarks, though curious in themfelves, remained without connection; and no one thought or ventured to publifh that the cryftallization of glafs, and cementation by the procefs of Reaumur are abfolutely one and the fame thing.

Experiments of Sir James Hall on whinftone, &c.

Sir James Hall,* in his valuable experiments upon whinftone and lava, publifhed in the 14th volume of the Bibliotheque Britannique, afcertained the property of thefe ftones to become fufed into glafs, and to return to the ftony ftate, according to the circumftances.

He did not purfue this devitrification among artificial products.

He called this laft fact a devitrification. He faw that it was the effect of a precipitation, and explained it in a manner that was highly fatisfactory and true; but being too much occupied in deducing from this fact arguments in favour of the volcanic geology, he neglected purfuing the interefting philofophical confequences to which the phenomenon pointed the way. This is

* Firft in the Edinburgh Tranfactions, and afterwards in our Journal. N.

 the

the taſk I have undertaken, and in the preſent memoir on devitrification, I ſhall give the reſults of my firſt experiments. As my ſituation gives me the uſe of a fire of extreme violence and continued for years together, it may have been in my power to make obſervations not within the reach of every one. The facts I ſhall report do partly explain themſelves; they are the reſult of laws to which all bodies are ſubjected. All the merit of the obſervation conſiſts in having ſeen them in ſubſtances, and on occaſions where it was not known that theſe laws do take place.

It is very remarkably ſeen in glaſs furnaces.

The bottom of the furnaces for fonding or fuſing the glaſs uſually preſents large cavities excavated by the action of the fire, and of the corroſive ſubſtances which often flow out of the melting pots. Theſe cavities are filled with a kind of glaſs called picadil, which is compoſed of aſhes which become vitrified, ſtones of the furnace which become fuſed, and more particularly of glaſs which falls from the pots. Care is taken to remove this at each font or making of glaſs. When the furnace is nearly worn out, the cavities having become large, cannot be entirely cleanſed, but ſome of the picadil remains. When the furnace is extinguiſhed, this picadil undergoes an extremely ſlow cooling, becauſe it is ſurrounded with maſonry of ſeveral cubic fathoms, which is itſelf penetrated with heat that has continued for upwards of a year. I have always remarked that it was in the glaſs of theſe bottoms of the furnace that I found cryſtallizations through the maſs of glaſs, the reſt of which was very tranſparent and pure. Theſe cryſtallizations, which were always conſiderably regular and numerous, excited my curioſity, as they had done that of other glaſs-makers before me. I collected many ſpecimens, taking care to chuſe thoſe which appeared the moſt curious, and preſented the moſt extrordinary characters.

Cryſtals in glaſs.

Examination of the facts.

Soon afterwards, from comparing the pieces themſelves, and the circumſtances under which they had been produced; and by the combinations of remarks, trials, and experiments to imitate theſe cryſtallizations at pleaſure, I at laſt ſucceeded in diſtinguiſhing various claſſes, all of which are produced by the nature of the different circumſtances which accompany the formation of the glaſs. I ſhall give a rapid outline of theſe, without entering into any remarks on the devitrification which almoſt conſtantly takes place in the ſcoria of forge furnaces.

This

This last fact must have presented itself to every one, and will without difficulty be referred to the observations I shall make.

The more compounded the glass the more easily it loses its vitreous nature.

The first remark which offers itself is, that the more compounded the nature of the glass, the more easily and readily it will become devitrified: but in the same manner as a solvent loaded with a great number of saline substances of different kinds, suffers them to crystallize more confusedly, so likewise it is found that these glasses do not present the most regular crystallizations. A precipitation takes place through the whole of the mass; each of the component parts obeys the laws of affinities; at the same time the transparency speedily disappears, and we no longer see a piece of glass, but a stone. Through this confused mass it is nevertheless impossible not to observe the rudiments of crystals. Such is the manner in which bottle glass comports itself in its devitrific action. These approach very much to the state of mere earthy glasses, having very little salt in their composition.

Bottle glass.

Experiment in the small way.

Every one has an opportunity of keeping a common bottle of black glass in a fire long continued, and capable of softening its paste; it speedily changes colour, becomes grey, and assumes the appearance of stone ware. In this manner we form the pottery of Reaumur, but without any process resembling crystallization.

Particular examination of the changes in glass by slow cooling.

But instead of observing the phenomenon in so small a mass, if we examine the bottoms of the glass furnaces in which these bottles are fused, we find that the glass is absolutely devitrified, and has assumed an appearance so completely stony, that the most experienced eye can scarcely distinguish the bricks of the furnace from the part which has been glass. It is only by following the course of the devitrification in pieces less advanced, that we succeed in distinguishing the glass in a granulated stone, which has rather the appearance of coarse pottery, or a strongly baked clay.

Instance of the effect taking place in a short time.

It often happens that the slow cooling of an hour or two is sufficient to produce the entire devitrification of bottle glass. I have pieces of eight centimetres ($2\frac{1}{2}$ inches) thick, which I obtained at the glass works of Mr. Saget, of La Gare. One of the pots was taken out of the furnace to be replaced; the glass remaining at the bottom of this pot was preserved from cooling during the time the pot itself acquired the common temperature, and by this means the nature of the glass was entirely

entirely changed. It no longer exhibits the tranfparency, but is a mafs of cryftals, compofed of fmall needles, converging towards common centers. It has no longer the appearance of glafs.

This fact fhews with what facility bottle glafs is devitrified, and always without the leaft appearance of cementation. The infinite variety of fubftances which make up the compofition of bottle glafs, greatly modifies the phenomena which take place during their devitrification, and muft, no doubt, influence the form of the cryftals; but my opportunities of obferving this kind of glafs have not been numerous.

Greenifh glafs of Alfatia is lefs compounded, and changes more flowly. Firft change, a bluifh colour.

Paffing therefore to glaffes lefs earthy and compofed of a lefs number of fubftances, when I make the fame examination of the bottoms of the furnaces of glafs for windows called glaffes of Alfatia, or half clear glaffes, into which is put a greater portion of pure fand and of alkali, I obferve nearly the fame phenomenon; but as they are lefs fudden, they are more eafy to diftinguifh and to feparate. At the firft inftant, and in the maffes where the devitrification commences, we feem to obferve a fmall portion of blue colour diffufed through a greenifh liquid.

A very fingular fact here prefents itfelf, which I fhall here only point out, with the intention of more fully examining it hereafter. This greenifh glafs mixed with blue, appears, in fact, to become of an obfcure blue when confidered by reflected light; but if placed fo as to tranfmit the light, it always appears greenifh; the blue being reflected and the green refracted, each fingly.

Second change, dull white opacity.

By continuing to obferve the devitrification of this glafs, it is foon perceived that the blue precipitation is followed by another more abundant, which affords a dirty white, and is very diftinct from the former. The colour of this glafs becomes deeper and deeper, and at length it refembles grey horn.

Third change, needled cryftals in groupes.

In thefe different tranfitions the pafte of the glafs appears conftantly to remain, diftinguifhable in its polifh, its fracture, and all its other properties, except its tranfparency. But in the midft of this pafte refembling horn, very diftinct cryftallizations are formed, confifting of nodules compofed of fmall needles, all converging towards the center. In this ftate it is no longer glafs, but a cryftal poffeffing all the phyfical properties of mineral fubftances left to themfelves.

An

An exact analyſis made of a certain number of theſe cryſtals carefully detached from the maſs, might indicate their nature and throw light on their formation.

It often happens that theſe cryſtallized nodules are enveloped in a cruſt which ſeems foreign to their nature, and may be compared to the cruſt which envelopes flints in the middle of the chalk-banks in which they ſeem to grow.

Speedy refrigeration confuſes the phenomenon.

Theſe are the ſeries of phenomenon attending the devitrification of greeniſh glaſs when it happens ſlowly; but if the effect is too rapidly produced, the appearances reſemble thoſe obſerved in the devitrification of bottle glaſs. The window glaſs here ſpoken of, is of that kind which contains very little of earthy matter, except wood aſhes. It muſt admit of many variations according to the differences in its compoſition.

Clear and ſimple glaſſes are not eaſily devitrified.

White or colourleſs glaſſes are very difficultly cryſtallized or devitrified. When they are well made, we may affirm that a long continued heat does not change them; but for this property it is neceſſary that they ſhould be compoſed merely of ſilex, with the preciſe quantity of flux for its ſaturation. In this caſe, a very long continued fire will no otherwiſe change them than by rendering them more yellow and hard.

(The Concluſion in our next.)

ADVERTISEMENT.

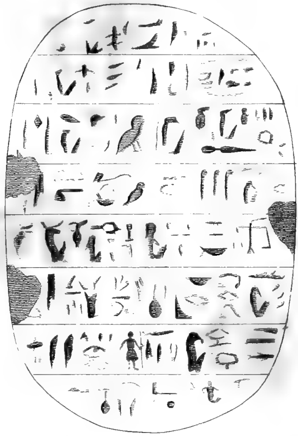
Various articles of Philoſophical News, Accounts of Books, and other ſubjects, have neceſſarily been poſtponed this month, on account of illneſs with which the Editor has been afflicted.

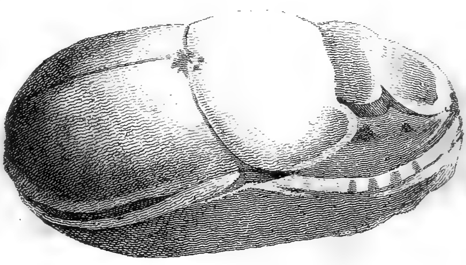
The ſame cauſe has alſo unfortunately prevented his Draughtſman from completing ſome Deſigns, which would elſe have appeared.

Mr. Ezekiel Walker has been prevented, by ſevere indiſpoſition, from anſwering the paper of C. L. in our laſt, upon the Horizontal Moon.

A note has alſo been received from C. L. in which he begs leave to notice, that the large lens mentioned in his experiment I. has a clear aperture of 6,1 inches in its frame, and was ſo uſed in that experiment.

Egyptian Engraving on Porphyry
probably astronomical.

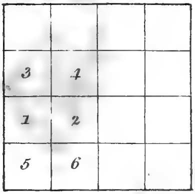




Mutlow Sc. Russell Co.t

Galvanic Illustrations.

Fig. 1.



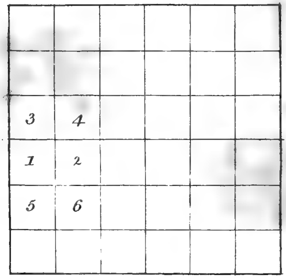


Fig. 2.

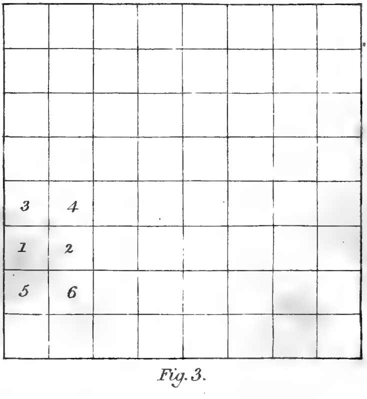


Fig. 3.

A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

FEBRUARY, 1805.

ARTICLE I.

Experiments and Remarks on the Augmentation of Sounds. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

A FACT is mentioned in a former communication of mine to your Journal (octavo, vol. III. p. 41) which proves, that a ftroke given to an extenfive vibrating furface by a flender rod, produces a found poffeffing a high degree of force: from this it follows, that the range of a found may be extended by enlarging the vibrating furface, while the magnitude of the impulfe remains the fame. The preceding maxim is of great ufe in acouftics; but as the truth of this propofition refts at prefent on the authority of a local obfervation, it may not appear fuperfluous to give a few eafy experiments in confirmation of the fact.

Sounds heard farther from an extended furface.

Experiment I.—My firft attempt endeavoured to prove, that when a found ceafes to be audible from remotenefs, the enlargement of the vibrating furface will again render it diftinct at the fame diftance. For this purpofe, a watch was fufpended, on a calm day, from the branch of a tree, about $5\frac{1}{2}$ feet from the ground. The clicking of it barely reached my ear,

Exp. 1. The beating of a watch audible at a greater diftance when in contact with a metallic plate.

ear, in thofe intervals of filence which were not difturbed by foreign founds, at the diftance of $5\frac{1}{2}$ yards; but when I removed half a yard farther, the ftrokes of the balance were no longer heard. A circular plate of rolled iron, one foot in diameter, was then brought into contact with the part of the watch fartheft from me, the pofition of it being fuch as to prefent the plane of the circle to my ear. This circumftance increafed the range of the found; for every ftroke of the inftrument was diftinctly perceived at the diftance of four yards, when the filence of the place remained undifturbed by other caufes. The refult of this experiment is too obvious to need a comment: and I have only to add, that when the trial is made within doors, the range of the clicking is greatly increafed; becaufe the primary found receives an acceffion of force from a number of its own pulfes, which are reflected to the ear by the furniture and walls.

Exp. 2. The plate gives a confiderable found itfelf.

Experiment II.—I proceeded, in the next place, to examine the effects produced on the auditory organs by the vibrations communicated to an elaftic furface by a watch; the immediate found of which was fmothered by art. For this purpofe, a watch was placed upon a cufhion, under an inverted porcelain cup, which was wrapped externally in feveral folds of flannel. The inftrument was heard with difficulty at the diftance of one foot in this fort of confinement; but when I placed the muffler with the watch under it, upon a fquare mahogany table four feet broad, the clicking noife imparted by it to the top of the table, reached the ear very diftinctly at the diftance of four yards. The fame watch, covered in like manner upon the iron plate mentioned above, reached my ear at the diftance of 22 feet. The apparatus ftood in this trial upon a round oak table, 24 inches in diameter.

The enlargement of the furface augments the found.

The preceding fact proves indifputably, that the fecondary vibrations of an elaftic furface actually affect the ear, in thofe cafes wherein the pulfes never reach the perfon of the hearers, which proceed from the part that is exclufively confidered as the feat of found; in other words it proves, that the caufe of a found is not confined to the obftacle receiving the primary impulfe, but that it is propagated from the point of impact through the contiguous bodies. The firft experiment alfo fhews, that the enlargement of the vibrating furface encreafes the force, which a given ftroke exerts upon an ear placed at

a given diſtance; conſequently the increaſed force thus imparted to a ſound, muſt be aſcribed to the co-operation of the vibrations communicated in the firſt experiment to the plate of iron; for by uniting with thoſe of the watch, they evidently augmented the power of the latter inſtrument.

Theory of the force of ſound augmented by the vibrations of a ſurface touching the ſonorous body.

A rational theory of the forces of ſounds may be formed, by help of the preceding obſervations, in the following manner: *Firſt*, Suppoſe the plane ABC *(Plate IV.)* to be one of the ſurfaces of an elaſtic body, and let it receive an impulſe at the point O; then O will recede from the ſtroke in a right line perpendicular to A B C, drawing after it all the contiguous particles, which, in their turn, will change the place of the next circle of particles. In this manner a circular dent O M E Q T F will be formed; the center of which O will be below the plane A B C; M, the higheſt part of the margin, will be above it; and the extreme circle E Q T F, bounding the whole, will lie in the plane. *Second*, Thus will a circular ſwell be formed on the plane, reſembling the circular wave produced by a ſtone dropped into water. A ſeries of theſe ſwells will follow the firſt, each of which, in ſucceſſion, will lie more remote from the center O than its predeceſſor. *Third*, The collective force of each ſwell is equal to that of the firſt impulſe; and it is diſtributed over the ſurface of a ring, having O for its center and O E for its breadth; *i. e.* all the rings are of equal breadths, but unequal diameter. *Fourth*, The effect of the ſtroke is thus progreſſively propagated from O to the more diſtant parts of the plane A B C, with an uniform velocity. *Fifth*, Let P be the place of the ear in the right line O P, perpendicular to A B C; alſo let G H I J K be any ſwell in the ſame plane; then will the effect of that ſwell be carried to the ear, in the conical ſhell O G I J K P; and the impulſe imparted to the auditory organs, through the medium of this ſhell, will be greateſt when the diameter of the ring O I is leaſt, and the contrary. *Sixth*, If A B C be a concave ſpherical ſurface, having P for its center, all the ſwells will ſucceſſively exert equal forces on the ear at P. *Seventh*, If O P be put equal to a, and f meaſure the force of a ſtroke at the diſtance 1; then let v equal the velocity with which this force is propagated from O over the ſpherical ſurface, and l the breadth of the rings covered by the ſwells, the force impreſſed on the ear at P in a given interval of time, is equal

to

to $\frac{fv}{la^2}$. *Eighth,* Though time is infinitely divifible in a mathematical fenfe, M. Euler has fhewn *, that the ear conceives it to confift of indivifible or elementary particles; confequently all the forces which arrive at P during one of thefe elementary intervals, make a fingle indivifible impreffion on an ear placed at that point; becaufe this organ cannot take cognizance of a fmaller particle of time; therefore the force of a found, produced at the center of a hollow fphere by the undulations of its furface, is truly denoted by the expreffion $\frac{fv}{la^2}$. *Ninth,* In general, the force of a found, at any inftant of its duration, is equal to the fum of the forces exerted by thofe pulfes, which ftrike the ear in the elementary interval: Confequently, if the diftance between the hearer and the founding body be fuch, that lines drawn from his ear to every point of the vibrating furface, may be confidered as equal, the force of the found will be expreffed by $\frac{fv}{la^2}$. *Tenth,* To fhew the nature of this theorem by an example; fuppofe a bell A, the diameter of whofe mouth $= 1\frac{1}{3}$, to be an octave below a bell B, the diameter of which $= 1$: alfo let A be heard twice as far as B, when each receives an equal ftroke; the ratio of the breadths of the fwells in A and B is required. Let $V, v,$ be the velocities of the fwells in A and B, $L, l,$ the breadths of thefe fwells; alfo let 2 and 1 be their refpective ranges: then fince f is given, $\frac{V}{4L} = \frac{v}{l}$, by the theorem; hence, as $V : 4v :: L : l$. Now the femi-circumferences of A and B are in the ratio of $1\frac{1}{3}$ to 1; but while the vibrations pafs over half the circumference of A, they pafs and repafs over the fame part of B: therefore, as $V : v :: 1\frac{1}{3} : 2$: hence alfo, as $L : l :: 1 : 6$. Q. E. I.

The principle extended to voices and other founds.

Should the principle explained above be admitted, the following conclufion muft alfo be received as a neceffary confequence of it: the voices of animals, as well as the notes of mufical inftruments, and the reports produced by blows given to lefs elaftic fubftances, derive no inconfiderable portion of their refpective forces from the vibrations of parts which are

* Tentamen de Mus. Cap. I. Sec. 13.

not

not directly concerned in the production of the primary ſound. This opinion appears to be countenanced by an experiment, which I have repeated at different times under various forms, and of which the following is the ſubſtance: If a wire be ſtretched by two pins fixed into a bad conductor, ſuch as a block of ſtone, the ſound produced by it is much weaker than that of an equal wire ſimilarly ſtretched upon a board, which is a better conductor. In like manner, if a circular piece of wood be ſtruck by a leaden ball conſtantly falling from the ſame height, the report will be heard at a greater diſtance, when the wood is placed upon a good conductor of ſound, than when it reſts upon a bad one. Theſe facts create a high degree of probability, that the leading maxim of this eſſay is applicable to ſounds of every deſcription, embracing ſuch as are continuous, as well as thoſe of a momentary duration.

Similar theory by Dr. Matthew Young.

Dr. Matthew Young's Enquiry into the Phenomena of Sounds has fallen into my hands ſince the foregoing remarks were committed to paper, and a partial peruſal of the work has convinced me, that the preſent theory has not all the claim to originality which I once ſuppoſed to be due to it. The juſtice and neceſſity of the preceding obſervation will appear from the Doctor's two theories of the ſpeaking-trumpet and echoes; in both of which he has made uſe of my leading principle with ſucceſs. The maxim, however, has been extended to a greater number of caſes by theſe obſervations; beſides which, an attempt has been made to demonſtrate the truth of it by experiment, and it is on the two circumſtances here ſtated that the merits of the preſent letter muſt reſt; for I am far from deſiring to rob the celebrated author of the enquiry of the honour due to his ſagacity.

JOHN GOUGH.

Middleſhaw, Jan. 4, 1805.

Obſervations

II.

Obſervations on the different Degrees of Facility with which Maſſes of the ſame Material admit of Changes in their Temperature; with Applications of the Facts to the Conſtruction of Pendulums, and Speculations upon various new Forms of pendulous Regulators of Time. In a Letter from J. WHITLEY BOSWELL, *Eſq.*

To Mr. NICHOLSON.

SIR,

Compound pendulums.

THE ſatisfactory communication on an ingenious improvement of the compound pendulum, in your Journal for December, has reminded me of ſome ideas which occurred to me on ſimilar ſubjects. In hopes that they may be of ſome uſe in a matter ſo intereſting in itſelf, and ſo important in its application, I ſend them for your opinion.

A very material point ſeems to have been omitted in all the conſiderations I have met with on the expanſion of bodies by heat; pyrometrical experiments being directed to that merely of different ſubſtances of the ſame ſize, but none being made on bodies differing in bulk.

Bodies are more or leſs ſpeedily expanded by heat according to their dimenſions and figure.

Though various bodies differ in their degree of expanſion by heat according to their materials, yet all are ſubject to certain laws on this point, depending on their dimenſions; for as bodies receive or communicate heat by their ſurfaces, and retain it in proportion to their bulk, it follows, that their mutability of temperature muſt depend on the ratio of their ſolid contents to their ſurfaces; and that the greater the ſurface in proportion to the bulk, the more readily will a body change its temperature; and on the contrary, the greater the bulk in proportion to the ſurface, the leſs will it be affected by the fluctuating heat of the ſurrounding medium.

Deduction of the effects.

The proportion which the ſurfaces of bodies bear to their bulk may be varied, either by altering their ſhape or changing their ſize. In the firſt reſpect it is ſufficient to note, that the flatter and longer any body is, the greater will be its ſurface in proportion to its maſs of matter, and *vice verſa*: the difference cauſed by the variation of bulk can be more eaſily calculated, as in bodies of ſimilar figures their ſurfaces are exactly

exactly as the ſquares, and their ſolid contents as the cubes of their diameters. From this, and what has been already premiſed, it will follow, that in bodies of different ſizes, ſimilarly ſhaped, and of the ſame ſubſtance, the capacity for heat will be as the cubes, and the mutability of temperature as the ſquares of their diameters or ſides; and that, therefore, the degree of the tendency of thoſe bodies to maintain an equality of temperature, may be eſtimated as their ſolid contents *minus* the relative value of their ſurfaces: Bodies whoſe ſhapes are diſſimilar, will in ſome degree be ſubject to the ſame rule; but it is not material to the preſent ſubject to take them farther into conſideration.

Table of the rapidity of heating and cooling.

The following table of the proportion of four cubes in the above reſpects, whoſe ſides are reciprocally as 1, 2, 3, and 4, will exemplify what has been above aſſerted, and ſhew how greatly the retention of temperature of bodies is increaſed by adding to their bulk.

	Side.	Surface.	Change of Heat as	Bulk.	Capacity for Heat.	Retention of Heat eſtimated.
	Feet.	Square Feet.		Cubic Ft.		
A	1	6 : 1	1	1	1	1
B	2	24 : 4	4	8	8	4
C	3	54 : 9	9	27	27	18
D	4	96 : 16	16	64	64	48

Inferences.

From theſe conſiderations the following inferences may be drawn: *1ſt*, That the greater the bulk of any body, the leſs will be its mutability of temperature in proportion, and of courſe the leſs will it alter its degree of expanſion: *2d*, That a large globe in the firſt place, or a cylinder, whoſe height was equal to its diameter, in the next place, or in the third place a large cube, would have its dimenſions very little changed by the fluctuations of atmoſpherical temperature.

Application of theſe facts to clocks.

To apply theſe principles to the regulation of horological movements may appear difficult, as the firſt idea that would occur is, that it would be neceſſary to put thoſe large bodies in motion for this purpoſe; but this is by no means needful, and it may be effectually performed by conſtructing and fixing up a pendulum in ſuch a manner, that its variations in length ſhall be corrected by, and depend on, thoſe of the large body.

There

Pendulums of Crofthwaite and Pine.

There is no need of much inveftigation to difcover the mode of doing this; as either the pendulum invented by Mr. Crofthwaite, of Dublin, or that contrived by Mr. Pine, will fully anfwer this purpofe, though they will by no means perform that for which they were intended,---of effecting a compenfation of themfelves, in the manner of the gridiron pendulum.

Their miftake.

The error of thefe gentlemen, in this refpect, is very fully pointed out in a paper figned A. B. publifhed in the feventh Volume of the Repertory of Arts, which would have been much more creditable to its author, had he not triumphed fo much in his fuperior penetration on the occafion, which has led him to forget himfelf fo far as to defcend to the illiberality of national reflection on the part of Mr. Crofthwaite: for which reafon I own I am happy to have it in my power to give thefe gentlemen the fatisfaction of feeing an error pointed out in his paper in return; which is, where he afferts that a fmall rod of mahogany muft have the fame contraction and dilation, from change of heat, as a *folid plank* of the fame wood, or words to that effect, the fallacy of which opinion appears from what has been here laid down: It would be eafy to diftinguifh this philofophy by an appellation of the fame ftamp as that which A. B. has beftowed on Mr. Crofthwaite's invention, and the more unfortunately for his infinuation on this occafion, as the Irifh method happens to be the beft of the two; for Mr. Crofthwaite fupported his pendulum by a folid wall, which would be much lefs affected by change of heat in the air than the back of the clock-cafe to which Mr. Pine attached his.

Ufe of a large block of ftone for fixing a pendulum and clock.

The beft method of applying the foregoing facts to the regulation of clocks appears to me to be, to procure a large cylinder, or octagonal prifm, of ftone, the diameter of whofe bafe is equal to its height, and exceeds the length of the pendulum by fome inches at leaft, and as much larger as it could be got the better: one of a cubic fhape would alfo do, if its mafs of matter exceeded that which could be got of the other forms; for this purpofe granite ftone feems preferable, and in the next place Portland ftone, as they can be eafily had in large blocks; but it is probable that ftones which grow damp in moift weather would not be fo proper for this purpofe. To this block of ftone a pendulum fhould be attached with a fingle

compenfating

compenſating rod added to it, in the ſame manner as that conſtructed by Mr. Croſthwaite or by Mr. Pine: the clock itſelf ſhould alſo be faſtened to the ſtone, particularly if Mr. Croſthwaite's pendulum is uſed.

The block of ſtone may be farther ſecured from change of temperature, by being ſurrounded on every ſide by brick-work, except where the pendulum and clock are fixed, and having dry ſaw-duſt rammed in between it and the brick-work. It is probable that a block thus fitted up, would vary little from changes in the heat of the air; and in thoſe ſituations where large blocks of ſtone are found naturally, this might be done at a ſmall expence: where a large block of ſtone could not be procured, a piece of dry ſtraight-grained red deal balk, or ſolid mahogany, might be uſed to good effect, if well painted, and incloſed as before directed: and for common uſe, it is probable that a large glaſs tube, filled with ſemen lycopodii, if to be had, or fine dry ſaw-duſt, well cloſed at either end, and covered with oiled ſilk, would form a pendulum-rod little affected by heat or cold; but if metal is preferred, then a tube of metal, fitted up as laſt directed, would be preferable to the ſmall wires now uſed for this purpoſe.

Defence of the block from change of temperature, &c.

The advantages to be expected from the principal method above-mentioned, of fitting up pendulums, is, that it affords an unlimited mode of approximation to perfect compenſation; and that, as it requires no great refinement of workmanſhip in its conſtruction, it can be uſed in many ſituations where it would be impoſſible to have a tubular compenſating, or common gridiron pendulum made; which, notwithſtanding the late improvements, are extremely difficult to conſtruct with accuracy, as may be ſeen in the paper publiſhed in your Journal for December laſt, where many of the impediments to their perfection are pointed out, and more ſtill remain to be noticed, of which the following deſerve ſome attention.

Advantages.

The metals of which gridiron pendulums are compoſed, are both of them mixed metals, and of courſe every different parcel of them made, muſt vary in ſome degree in the relative quantities of their component ingredients, and from thence in their degree of expanſion by heat. It may appear ſtrange to call ſteel a mixed metal; but when it is conſidered that charcoal forms a conſiderable part of it, the juſtice of eſtimating it as ſuch will be evident; and in this metal the proportion

Imperfections of compound pendulums ſtated. The metals are mixed, and not conſtant in their compoſition.

portion of charcoal in different parcels of it, is much more various than that of zinc is in brafs: perhaps, for this reafon, it might be better to ufe fome of the fimple metals in the place of thefe, in their conftruction.

Difficulty of adjuftment.

Another difficulty in forming thefe pendulums with accuracy, arifes from the want of a good method of proving them. The adjuftment by the going of a clock is fhewn to be imperfect in the paper on the tubular pendulum, and that propofed in its place, the ufe of the pyrometer, is equally defective, for the following reafons:

Defects alledged in pyrometers.

Pyrometers are unfit for meafuring the effects of the heat and cold of the atmofphere on any fubftance, becaufe their own machinery, and particularly the part fupporting the fubftance under trial, is liable to be affected by the fame caufes: and in this cafe the index will fhew the fum of the alteration of the fubftance, the part of the apparatus between its two extremities, and the machinery of the pyrometer, inftead of that of the fubftance alone: and in applying artificial heat to the matter under trial, it is extremely difficult to communicate that heat equally to all its parts at the fame time, and fo as not to operate on the pyrometer itfelf, without which the exactnefs of a compound pendulum could not juftly be tried by it.

Laftly, another error is produced in the computation of the aberration of all pendulums, from not taking into account the dilation and contraction of the fmall fteel fpring by which they are ufually fufpended.

Other pendulums.

As all the ofcillatory pendulums yet made public are influenced in their notation of time by the expanfion of their fubftance, and as the beft contrivances to compenfate this are only an approach to perfection, an increafe of probability but no certainty, it is therefore an object worthy attention to inveftigate other methods for effecting the fame purpofe: for which reafon I fhall here beg leave to fuggeft the reconfideration of a fpecies of pendulum of a different nature, which has never been condemned on any juft ground that I could hear, and which in fact is fo little known, that the application of a fimilar movement to other purpofes, is by many fuppofed to be a late invention. The pendulum which I allude to, is that treated of in the fifth Part of the *Horologium Ofcillatorium* of the well known Huygens, publifhed in 1673, which he calls

calls the circular pendulum, and which much refembles the centrifugal apparatus, ufed frequently about fteam-engines to regulate the aperture of the fteam-damper, of which it probably fuggefted the firft idea. To form the circular pendulum, a fpindle (H D, *Fig.* 1, *Plate III.*) proceeds perpendicularly from the clock-work, from whence it receives a circular motion round its own axis; to this is affixed a lamina (B G A) of fome breadth, bent according to a paraboloidal line, by the evolution of which (after it is joined to a certain right line) a parabola is formed, the conftruction of which is fhewn in the eighth propofition of the third part of the above work: This lamina caufes the ball of the pendulum (F), attached by two threads to its upper extremity (as it circulates), to perform all its revolutions (which will be of greater or lefs extent as the axis moves with greater or lefs force) in the furface of a conoidal parabola (F E). Huygens declares, that all the circles performed by the pendulum thus conftructed, will be ifynchronous, and then fhews how to proportion the parts of the apparatus, fo as that each of its revolutions fhall be performed in a fecond, or in half a fecond: He fays, the only reafon which caufed the ofcillatory pendulum to be preferred to this was, that this laft was more difficult to conftruct. As this circular pendulum feems to poffefs the valuable property of correcting the alterations caufed in it by expanfion, or of rendering them of no confequence, the difficulty of conftruction is a matter of no confequence, efpecially as there are now to be found artifts fo much more excellent in works of this kind than formerly.

Circular pendulum of Huygens.

Huygens gives the following character of this pendulum, which I tranfcribe in his own words: "*Plura tamen, hujus quoque generis de quo nunc loquimur, nec fine fucceffu, conftructa fuere: eftque in his fingulare illud, quod continuo atque æquabili motu circumferri cernitur index poftremus, qui fecunda fcrupula defignat, cum in omnibus aliis horologiis fubfultim quafi feratur: Item hoc quoque, quod abfque ftrepitu fonoque omni moveantur hoc ratione conftructa automata.*" He concludes the book with thirteen theorems, *De vi centrifuga ex motu circulari*, feveral of which prove this kind of pendulum to have moft valuable qualities: the fixth of thefe, being very remarkable, I copy for your readers, who may not have an opportunity of feeing the work which contains it, as follows:

"*THEOREMA.*

" *THEOREMA.*

" *In cava ſuperficie conoidis parabolici, quod axem ad perpendiculum erectum habeat, circuitus omnes mobilis, circumferentias horizonti parallelas percurrentis, ſive parvæ ſive magnæ fuerint, æqualibus temporibus peraguntur : quæ tempora ſingula æquantur binis oſcillationibus penduli, cujus longitudo ſit dimidium lateris recti parabolæ genitricis.*"

From the above theorem, and what has been already laid down on the ſubject, it is evident that the circular pendulum is well worthy of a fair trial, and ſhould be recommended to the attention of all ſcientific conſtructors of horological movements.

As this circular pendulum will take up more room than a common one, when this is any conſiderable inconvenience, one of the following conſtructions on the ſame principles (which have occurred to me while writing this communication), may be uſed in its place.

Various conſtructions of the circular pendulum.

The firſt is as follows: To a ſhort ſpindle aſcending from the clock, as before deſcribed, let there be attached a glaſs or iron tube, bent in the form of the conoidal parabola beforementioned, and placed as E F in *Fig.* 1 : into this tube let there be poured a ſufficient quantity of mercury to ſerve as a centrifugal weight.

It appears to me that the mercury riſing in the tube, as it circulates, by the centrifugal force, along its parabolical curve, will have the ſame effect as the weight in Huygens's conſtruction, cauſed by other means to move in a ſimilar line. The tube ſhould be ſufficiently large to admit the air to paſs freely above the mercury as it moves along its cavity, or elſe a ſmall aperture ſhould be made in the upper ſurface of the tube near the ſpindle, for the ſame purpoſe: to prevent alſo the error which might otherwiſe be cauſed by a varied reſiſtance to the motion of the circulating tube from the mutability of atmoſpherical preſſure, the tube may be incloſed in a circular veſſel covered at top, made of as light materials as poſſible, and very ſmooth externally, through whoſe axis the ſpindle ſhould paſs, and to which it ſhould be united ſo as to revolve with the tube. A ſecond method of effecting the ſame purpoſe is, to have a ſemi-cylindrical trough ſhaped and placed in ſame manner as the above tube, in which ſhould be put a metallic ſphere of ſmaller diameter than the trough, that the ſphere

ſphere might move in it without friction; for, being thus formed, it follows, from the 13th of the third book of Euclid, that the ſphere could never touch the trough but in one point alone at the ſame time. A third method of conſtruction is, to place a cylindrical metallic rod, or thick wire, bent into the parabolical curve before directed, in the ſame poſition as the above tube and trough; with a ſpherical weight put on it ſo as to move freely along it, which laſt might be effected by a friction-roller, either let into the upper part of the weight, or placed above it, whoſe ſurface ſhould be formed into a circular groove of larger diameter than the rod.

Thoſe laſt conſtructions ſeem to me to have beſides the farther advantage, that any dilation of their parts from change of temperature, could only lengthen the tubes or rods, but would not alter their ſhape, on which alone their properties depend; which circumſtance might perhaps make them even more exact than that propoſed by Huygens, the ſtrings, or appending part of which, would certainly vary in length; which, though, from the before-recited theorem, it appears to be a matter of little conſequence, yet Huygens ſeems to hint that it would be of ſome, by recommending the uſe of fine chains inſtead of ſtrings (that their length might vary the leſs), in theſe words: "*In locum fili catenulam tenuem exauro, aliove metallo, adhibere licebit, quo melius invariata ſervetur longitudo.*"

An oſcillatory or rolling pendulum.

There has alſo occurred to me a ſpecies of oſcillatory pendulum; which, as it appears to have the ſame property as thoſe laſt mentioned, (of not being affected by the expanſion of its materials,) I ſhall here deſcribe: It conſiſts of a cylinder of hard metal, A (*Fig.* 4) turned very true and ſmooth, placed ſo as to roll back and forwards alternately, in the cycloidal cavity B, cut in a block of hard metal alſo, and well poliſhed: the communication of motion between the rolling cylinder and the clock work to be effected by the rod C, ſuſpended by its upper extremity, and connected with each extremity of the axis of the cylinder by the joints D D: The diameter of the generating circle of the cycloid being half the length of a pendulum rod, which vibrates in the required time, and the cycloidal part well levelled and firmly ſuſtained. It ſeems to me that the cylinder A, when put in motion, will oſcillate equal times on its cycloidal ſupport; and, (as the expanſion of the latter will not alter its ſhape, on which alone its properties depend,)

pend,) that the refult will be the fame in every different temperature; B may alfo be formed of two cycloidal plates, having circular grooves hollowed on their edges, and A have two projecting circles turned on it to run in thofe grooves; the method of connecting A with the clock-work may alfo be varied many other ways.

Concerning friction, &c.

Before I conclude, I beg leave to obferve that the method, before mentioned, of preventing friction in the motion of the fphere in the parabolical trough, might be applied very beneficially in the conftruction of rail roads, by forming the upper part of the rail of a femi-cylindrical fhape, and making in the edges of the wheels, (which were to run on them,) circular grooves, whofe diameters fhould each exceed that of the rail; in the ufual way of forming rail-roads, the lateral friction to the wheels is very confiderable, whether they have flanches to them, and the rails are made plain, as in the Swanfea roads; or the rails are formed with rifing ledges, and the wheels plain, as in the Croydon road.

The circular pendulum a good regulator of firft movers.

I fhall alfo requeft permiffion to mention, in addition to your note on Prony's condenfer of forces, that a regulator might be formed on the principles of the circular pendulum, as *Fig.* 3, which would temper the motion of wind-mills, or other engines, and react on them in return, (as well as the fly wheel), in a much more fimple and effectual manner than the very complicated apparatus defcribed by Prony for the fame purpofe, and which feems to me moreover peculiarly liable to be broken by any fudden increafe of velocity in the mill, unlefs formed of moft cumberous ftrength.

I requeft the favour of having the following typographical errors noted in the paper on my tallow lamp. November, 1804, page 147, line the eighth, for the word *frefh,* read *freely*; and line the 25th, fame page, between the words *means* and *horizontal,* infert the letter P. In the plate alfo, the pan fhould be deeper and larger than there reprefented, and the tallow holder more central.

I am, Sir,

Your very humble fervant,

J. WHITLEY BOSWELL.

Reference

Reference to the Figures.

Fig. 1. An exact copy of the figure of Huygens's circular pendulum, from the Horologium. B G A, the edge of the paraboloidal lamina: F E, the parabola produced by its means: F, the ball.

Fig. 2. The first construction proposed in place of *Fig.* 1. A C, the parabolical glass tube: A, the mercury: C, the air-vent: B B, the enclosing circular vessel represented in section.

Fig. 3. The third construction proposed instead of *Fig.* 1. A C, the metallic rod bent into a parabolical figure: B, the spherical weight: V, the roller on which it moves along A C: the parts in this figure are represented double, to shew the best method of adopting this plan as a regulator for wind-mills and other engines.

Fig. 4. The proposed construction for an oscillatory pendulum, which, it is imagined, will not be affected by change of temperature. A, the oscillating cylinder: B, the cycloidal support.

Fig. 5. A section of the trough mentioned in the second proposed construction, with the sphere in it.

Annotations. W. N.

The best pyrometers do not give fallacious results.

Deluc's method.

Pyrometers, p. 74.] The common pyrometers of the shops are indeed liable to the objections of our author; but ingenious men have long ago obviated them in their experiments. Deluc, in the Philosophical Transactions for 1777, gives a method of ascertaining the relative expansions of two different metals by heat. He suspended one of the bars to an arm proceeding horizontally from an upright deal plank, and he supported the other bar by resting its lower end upon a small cock or stage proceeding from the lower end of the former. A microscope was attached to the plank in such a manner that, while it was constantly supported by an horizontal arm, it could be moved so as to keep the last mentioned or standing bar in the focus of radiation for distinct vision. Heat was applied to the bars, and the microscope was directed to a point on the standing bar. When by repeated shifting the microscope a point was found which was neither raised nor depressed by the changes of temperature, the respective lengths of the bars were inversely as their expansive powers. There was no mechanism, and the plank would remain at the same temperature during the experiments,

riments, or might be kept fo by means fimilar to thofe pointed out by our ingenious author, or his block of ftone itfelf might have been ufed.

Ramfden's pyrometer.

That great artift Ramfden, whofe mechanical fkill and clearnefs of intellect were fo varioufly difplayed in his works, and ftill more in his converfation to thofe who remember and regret that he has recorded fo little of the refults of his labours: this artift firft availed himfelf of microfcopes for afcertaining the terminal points of the fubject under examination by his pyrometer; and the interval between one microfcope and the other was rendered permanent by fixing them to arms proceeding at right angles from a bar of iron, kept at 32° by furrounding it with melting ice. Phil. Tranf. Vol. LXXV.

Methods of examining gridiron pendulums.

Magellan's inftrument.

Laftly, another error, p. 74.] I believe the fmall fpring and fimple bar part, if any beneath the gridiron, are always taken into the account. Thefe pendulums have not unfrequently been fubjected to actual examination. An old friend of mine, Mr. J. H. de Magellan had an apparatus of tin, (Anno 1784) in which he inclofed the gridiron when to be examined. It was hung by its fpring to an arm fixed in an upright plank of deal wood, againft which plank were attached thermometers to fhew its fteadinefs of temperature; and from the center of ofcillation proceeded a (temporary) rod, the end of which prefented a dot as the object to be viewed through a microfcope duly attached to the plank. Without dwelling minutely on this apparatus, I need only obferve that neither the arm of fufpenfion, nor the fupport of the microfcope, nor the rod from the center of ofcillation had any parts but fuch as were parallel to the horizon, while the pendulum itfelf was perpendicular to it, and confequently that no error could arife but from expanfion in the plank, which remained at the common temperature; and that the tin apparatus which covered and enclofed the pendulum had only three openings, one at the bottom, one at top, and one through which the rod from the center of ofcillation paffed, without touching the fides. In the experiment, fteam from boiling water was admitted below, and when the heat was fo raifed, and the fupply kept up, that uncondenfed fteam efcaped above the pendulum was judged to be at 212°, and if the center of ofcillation continued before the microfcope without either rifing or falling, the compenfation was confidered as complete.

Mr.

Mr. Troughton's apparatus, alluded to in his paper is different from thofe, and when he fhall favour me with the account of it, and the experiments he is making, the world will fee that it anfwers its purpofe with fuch facility and precifion as do honour to his great fkill in thefe fubjects.

Troughton's pyrometer:

Circular Pendulum of Huygens.] The difficulties attending the conftruction of this pendulum feem to be principally in the ftring or chain. Perhaps fine metallic wire might deferve the preference. It would not probably be difficult to bring the curve A B fufficiently near the true figure to anfwer its general purpofe, through the changes of force in the firft mover, and of refiftance in the air. Practical men have not thought the cycloidal cheeks applied by Huygens, and fince him by others, to the common pendulum, of any utility; but have rather directed their attention to fmall vibrations, or an equalizing of the firft mover by periodically detaching the train. Huygens himfelf alfo propofed this expedient. It is generally admitted to be an advantage that the regulating inftrument, whether pendulum or balance, fhould perform the greateft part of its motion unconnected with the train. The circular pendulum is conftantly fo connected.

Remarks on Huygens's circular pendulum.

As the ingenious author has not detailed his reafons for thinking that pendulums of this kind will not be affected by change of temperature, I would fimply remark that it does not appear to me why an expanfion of the ftring fhould not caufe the pendulum of Huygens to revolve more flowly, or that an expanfion of the curves in the other figures would not diminifh their curvature and produce the fame retardation.

Whether it be lefs liable to change from temperature.

The tube may be enclofed in a circular veffel.] As the refiftance of the air will vary no lefs than a tenth or a fifteenth part according to the ftation of the barometer, we might expect time pieces to be confiderably affected by its irregularity. In aftronomical clocks, with heavy pendulums and fhort vibrations, this quantity is extremely minute, as is proved by their very correct performance. But there are facts attending the performance of our beft portable chronometers, which fhew that it is not in them an inconfiderable object. Several years ago I entertained a notion that the refiftance of the air might be rendered equable, or in fact done away, by boxing up the balance, as is here propofed by our author. But from his own excellent paper on the blaft ventilator inferted in the

On the method of enclofing the pendulum, &c.

fourth volume of the quarto feries of this Journal, as well as from thofe of Profeffor Venturi, inferted in the fame work, I learned how greatly the lateral reaction of the air, which is put in motion by bodies paffing through it, muft affect their movements. From this caufe the value of the contrivance will undoubtedly be diminifhed, but I think not done away. I believe the famous pocket time-piece made about the year 1784, by Emery of Charing-Crofs, for the Prefident Saron at Paris, had its balance boxed up; but as I fpeak only from recollection, it is quite as probable that the whole movement was enclofed in a cap or cafe fhut up by a fecret catch.

III.

Analytical Experiments and Obfervations on Benzoin. By Mr. WILLIAM BRANDE'. *Communicated by the Author.**

Benjamin or benzoes little examined.

THE fubftance which forms the fubject of the following experiments, and which is generally termed Gum-Benjamin or Benzoes, may be enumerated, among thofe objects, which till lately, have but little engaged the attention of chemifts.

Mr. Hatchett's experiments.

We are indebted to Mr. Hatchett for almoft all that is as yet known, as to the chemical properties of the refins, gum-refins and balfams; and as the fubftance in queftion, had not been examined by that gentleman, I was induced to make the following attempt.

Benzoin is obtained from the ftyrax benzoe.

Benzoin is obtained by incifions made in the tree called *Styrax Benzoe* †, from whence the balfam flows, one tree fcarcely ever yielding more than three, or at the moft four pounds.

Two kinds: viz. benzoë amygdaloides, and

Benzoin is generally divided into two kinds; the one is formed of whitifh yellow tears, refembling almonds, united

* The following experiments were made merely with a view of afcertaining the effects of different menftrua on benzoin; but as the action of fome of the acids, &c. would have been much lefs interefting, without a knowledge of the component parts of the balfam, I was induced to make fome analytical experiments, which, though I fear they are by no means fo accurate as they ought to be, will, I hope, ftill be found of fome ufe.

† Dryander has given a defcription and drawing of this tree, *vide* Phil. Trans. 1787. p. 307.

together

together by a brown ſubſtance of the ſame nature, and is diſtinguiſhed by the name of benzoë amygdaloïdes.

The other is a brown ſubſtance, ſomewhat reſembling common reſin, but poſſeſſing in other reſpects, the ſame qualities as the former; it is called common benzoin. common benzoin.

The difference between theſe two ſpecies is ſaid to ariſe from the latter having been expoſed, for a length of time, to the ſun, which converts the white benzoin into the brown. Cauſe of this difference.

The benzoin of the ſhops is uſually in very large brittle maſſes, and ſuch as is whiteſt, and free from extraneous ſubſtances is the moſt eſteemed. It grows in Sumatra, and is brought from the Eaſt Indies only. Benzoin of the ſhops.

When chewed, it impreſſes a ſlight ſweetneſs on the palate; it has however but little taſte. Its ſmell is fragrant and very agreeable, becoming much more perceptible when gently heated. Taſte. Smell.

When thrown on hot coals it firſt fuſes, then takes fire, emitting at the ſame time a ſtrong penetrating odour. Its ſpecific gravity, according to Briſſon, is 1.092. Effects of heat on benzoin. Sp. gr.

Gren enumerates it among the reſins *, as do moſt other authors of his time; it is however more properly denominated a balſam, ſince it is a combination of reſin and benzoic acid. Compoſed of reſin and benzoic acid.

We ſhall firſt examine the effects of different menſtrua on benzoin, and then proceed to its analyſis, together with the methods of obtaining benzoic acid.

§ I.

Effects of different Menſtrua on Benzoin.

1. Cold water has but very little effect on benzoin; boiling water however extracts a part of its acid. Effects of water on benzoin.

2. Alcohol diſſolves the whole of benzoin when digeſted with it in a very gentle heat, the impurities remaining behind. The ſolution is of a deep yellow colour, when perfectly ſaturated, inclining to reddiſh brown, and poſſeſſing in ſome degree the fragrant ſmell of the balſam, which may be obtained pure, either by gentle evaporation of the ſolution or by the addition of water, when a white powder precipitates, formerly known by the name of magiſtery of benzoin. Of alcohol.

The actions of the acids on the ſolution of benzoin in alcohol, are very deſerving of attention. Action of the acids on this ſolution.

* Handbuch, § 1174. 2nd edition. 1794.

 A. Muriatic

Of muriatic acid. A. Muriatic acid being added, a white curd is formed, and when three parts of acid and one of the ſolution are heated together, a very ſmall part of the balſam is rediſſolved, which ſeparates as the mixture cools, while the remainder is converted into a black brittle ſubſtance when cold, part of the benzoic acid having been diſſipated by the heat and part diſſolved, ſome of which ſeparates in the form of a white ſcum, as the mixture cools, together with the benzoin.

Of ſulphuric acid. B. Two or three drops of ſulphuric acid added to the alcoholic ſolution, occaſion a white precipitate, which diſappears on the addition of a ſmall quantity more of acid, the mixture aſſuming the appearance of port wine.

If however equal parts of the ſolution and of ſulphuric acid be mixed together, a dark pink precipitate is formed, the fluid becoming likewiſe of the ſame colour, but the addition of water changes the whole to a lilac colour.

On evaporating a mixture of equal parts of ſolution of benzoin and ſulphuric acid, the liquid retains its beautiful red colour till towards the end of the proceſs, when it becomes gradually darker, and if the evaporation be carried on to dryneſs, a black coaly ſubſtance remains, a decompoſition of the balſam having taken place.

Of nitric acid. C. Nitric acid added to the ſolution of benzoin in alcohol forms a dark red fluid, and when equal quantities are mixed, a violent efferveſcence takes place, attended with the emiſſion of much nitrous gas. It muſt be obſerved that in this inſtance, no precipitate is formed, which proves that the balſam is ſoluble in nitric acid.

Of acetous acid. D. The effects of acetous acid do not exceed thoſe of water.

Of acetic acid. E. Acetic acid forms a precipitate when added to this ſolution of benzoin. The effects produced by acetic acid on benzoin will be noticed hereafter.

Of the alcalies. F. The alcalies form no precipitate in this ſolution of benzoin, although the mixture in ſome caſes becomes ſomewhat turbid.

Action of ether on benzoin. 3. Ether diſſolves benzoin with great facility, the balſam being ſeparated, when this ſolution is agitated with water.

Effects of the acids on this ſolution. The effects of the acids on this ethereal ſolution, ſo nearly coincide with thoſe produced on the ſolution in alcohol, that there will be no need of a detailed account of them.

4. Nitric

4. Nitric acid produces a violent effervefcence when poured upon powdered benzoin, the balfam being at the fame time converted into an orange coloured mafs. Effects of nitric acid on benzoin.

Six ounces of nitrous acid, of the fpecific gravity of 1.36 being poured on one hundred grains of very pure benzoin reduced into powder, the mixture being gradually heated in a fand bath till it boils, a folution of a light yellow colour is formed. The folution thus obtained, depofits a fmall quantity of benzoic acid, as it cools: this quantity gradually increafes, till after fome days, the whole of the benzoic acid appears to have been feparated. Solution. Separation of benzoic acid from,

This fact was firft obferved by Gottling; he does not however feem to have obferved that the whole of the balfam is foluble in nitric acid. firft obferved by Gottling.

A. When the above folution, recently made, is poured into water, the benzoin is precipitated, apparently unaltered. Effect of water on the nitric folution,

B. Sulphuric acid caufes no alteration in this folution. of fulphuric acid,

C. Muriatic acid forms a white precipitate which is rediffolved on the application of heat, forming a bright yellow liquid, which is a folution of benzoin in nitro-muriatic acid, and in which no precipitate is formed either by water or the alcalies. Its colour is converted into dark brown, when excefs of potafh is added. of muriatic acid. Sol. of benzoin in nitro-muriatic acid. Effects of water and the alcalies on the nitro-mur. folution,

5. When fulphuric acid is poured on pulverifed benzoin, an effervefcence takes place; part of the benzoin is diffolved; forming a deep red liquid, and at the fame time a coaly fubftance remains on the furface. Sulphureous acid gas is difengaged during the folution. Effects of fulphuric acid on benzoin.

A. The alcalies form no precipitate in this folution, till after fome hours ftanding, when a dark coloured precipitate is formed. Effects of the alcalies on this folution,

B. Water produces a lilac precipitate in the recent folution. of water.

6. Neither muriatic nor acetous acids have any effect on benzoin. Effects of muriatic and acetous acids on benzoin,

7. Acetic acid diffolves benzoin, even in the cold. The folution formed by the affiftance of heat becomes very turbid on cooling, owing to the feparation of the benzoic acid. of acetic acid.

A. The alcalies form a white precipitate in this folution.— The precipitate formed in the alcoholic folution, *(page 11)* is re-diffolved after fome hours ftanding. Effects of the alcalies on this folution.

8. Acetic ether diffolves benzoin, without the affiftance of heat. Effects of acetic ether on benzoin.

9. When

Effects of soda and potash on benzoin.

9. When a boiling lixivium of pure potash is poured on benzoin, a solution is immediately formed; the same effect is produced when pure soda is made use of. These solutions are of a dark brown colour, and become turbid after some days exposure to the air.

Effects of the acids on these solutions.

Precipitates are formed in them by the acids, which are redissolved on the application of heat, when nitric or acetic acids are added in excess.

Effects of ammonia on benzoin.

Ammonia likewise dissolves a small quantity of benzoin, from which solution it is precipitated by the acids.

Effects of water on the alcaline solutions.

No alteration is produced when the alcaline solutions are largely diluted with water.*

§ II.

ANALYTICAL EXPERIMENTS ON BENZOIN.

Distillation of Benzoin.

Distillation of benzoin.

One hundred grains of very pure benzoin were put into a glass retort, to which a tubulated receiver, and pneumatic apparatus were adapted; a very gentle heat was first applied, which was afterwards gradually increased, till the bottom of the retort became red-hot. The products thus obtained were as follows.

Component parts of benzoin.

	Grs.
Benzoic acid - - - - - - -	9.
Acidulated water - - - - -	5.5
Butyraceous and empyreumatic oil - -	60.
Brittle coal - - - - - - - -	22.
And a mixture of carbonated hydrogen and carbonic acid gas, which may be computed at -	3.5
	100.0

The quantity of benzoic acid above mentioned is that which may be separated by sublimation only, for on treating the oil, (which amounts to 60 grains) with water, five grains more of acid may be obtained, so that 100 grains of benzoin contain

Oil - - -	55
Acid - -	14

* For an account of Mr. Hatchett's experiments on the resins, gum resins, and balsams, see Thompson's System of Chemistry, 2d edition, Vol. IV. page 305. for the resins and balsams to page 328. And from page 341 to 347 for the gum resins.

 The

Chaptal's method of obtaining benzoic acid.

The greateſt quantity of benzoic acid is obtained by Chaptals method, which conſiſts in diſtilling all the products over together, and ſeparating the acid by means of boiling water.

Scheele's proceſs.

The acid, however, thus obtained, is by no means ſo pure as that procured by Scheele's proceſs *, which is certainly far preferable in many reſpects.

Gottling's proceſs for obtaining benzoic acid.

Another way of obtaining the benzoic acid, is that recommended by Göttling. It conſiſts in digeſting the balſam, in a gentle heat with carbonate of potaſh and precipitation by ſulphuric acid. Gren made uſe of carbonate of ſoda: but it muſt be obſerved that in both theſe proceſſes, the acid is by no means ſo pure as that obtained by Scheele's method, becauſe a far larger proportion of the benzoin itſelf is ſoluble in the fixed alcalies, than in lime water, (the latter only diſſolving a very ſmall portion, to which its brown colour is owing) and

Gren's proceſs.

* Scheele's proceſs is as follows: "Upon four parts of unſlacked lime pour 12 parts of water, and after the ebullition is over add 96 parts more of water; then put 12 parts of finely powdered benzoin into a tin pan; pour upon it firſt about ſix parts of the above milk of lime, mix them well together, and thus ſucceſſively add the reſt of the mixture of lime and water. If it be poured in all at once, the benzoin inſtead of mixing with it, will coagulate and run together into a maſs. This mixture ought to be boiled over a gentle fire for half an hour, with conſtant agitation; then take it from the fire, let it ſtand quiet for an hour, in order that it may ſettle; pour off the ſupernatent limpid liquor into a glaſs veſſel. Upon the remainder in the pans pour 96 parts of pure water, boil them together for half an hour, then take it from the fire and let it ſettle; add the ſupernatent liquor to the former; pour upon the reſiduum ſome more water, boil it as aforeſaid, and repeat the ſame proceſs once more. At laſt put all the reſiduums upon a filter, and pour hot water ſeveral times upon it. During this proceſs, the calcareous earth combines with the acid of benzoin, and ſeparates it from the reſinous particles of this ſubſtance. A ſmall quantity of the reſin is diſſolved by the lime water, whence it acquires a yellow colour. All theſe clear leys and decoctions are to be mixed together and boiled down to 24 parts, which are then to be ſtrained into another glaſs veſſel. After they are grown cold, muriatic acid is to be added, with conſtant ſtirring, till there be no further precipitation, or till the maſs taſtes a little ſouriſh. The benzoic acid which was before held in ſolution by the lime, precipitates in the form of a fine powder." Vide Thomſon's Chemiſtry, Vol. II. page 123.

which

which is precipitated by the diluted ſulphuric acid. The following table ſhews the quantity of benzoic acid, obtained by the different proceſſes, from one pound of benzoin.

	Oz.	Dr.	Scr.	Grs.
Scheele's proceſs - - - -	1	6	2	19
Chaptal's - - - - - -	2	0	0	0
By boiling benzoin with water - -	1	0	0	10
By Gren's and Gottling's proceſſes -	1	5	1	10*

The acid of benzoin, firſt deſcribed by Blaiſe de Vigenere.

Benzoic acid is deſcribed by Blaiſe de Vigenere, as long ago as the year 1608, in his treatiſe on fire and ſalt; he called it flowers of benzoin, becauſe it was obtained by ſublimation. We are indebted to Tromſdorf and Lichtenſtein for many facts relating to this acid.

Properties of the Acid of Benzoin.

Taſte of the acid. Smell.

This acid when obtained according to Scheele's proceſs, is a light yellowiſh powder. Its taſte is hot and rather bitter. It poſſeſſes a ſomewhat fragrant ſmell, ariſing from a ſmall portion of the aromatic oil, which ſtill adheres to it, it has however been obtained without any ſmell by Gieſe.

Properties in general.

It reddens tincture of turnſole. It is volatilized by a gentle heat, being converted into a white ſmoke, which excites a very diſagreeable ſenſation in the throat. When melted, it becomes as fluid as water, and aſſumes a radiated ſurface on cooling. Its ſpecific gravity is 0.667.

Decompoſition.

When diſtilled in cloſe veſſels, a part of it is decompoſed, which is converted into oil and carbureted hidrogen gas.—

Effects of oxigen gas, &c. on this acid.

Tromſdorf † found that it was not altered by oxigen gas, nor by the ſimple combuſtibles or incombuſtibles.

Of ſulphuric acid.

It is ſoluble in ſulphuric acid, which it converts into a brown liquid, and from which it is ſeparated by the addition of water.

Of nitric and ſulphurous and nitrous acids. Effects of muriatic acid, &c.

The ſame effects are produced by nitric acid, and by ſulphureous and nitrous acids. It is not acted upon by muriatic, oxymuriatic, or phoſphoric acids. It is eaſily ſoluble in alcohol, from which it is precipitated on the addition of water. Its

* Of pure acid, obtained by treating the precipitate with boiling water.

† See Tromſdorf's experiments on the benzoic acid, in his Journal der Pharmacie.

affinities

affinities appear to be as follows:*—White oxide of arſenic, potaſs, ſoda, ammonia, barytes, lime, magneſia, alumina. Its affinities.

Properties of the Oil of Benzoin.

The oil obtained by the diſtillation of benzoin poſſeſſes a ſtrong empyreumatic odour; but when rectified by a ſecond diſtillation, its ſmell is exceedingly fragrant and pleaſant. Its taſte is acrimonious and very diſagreeable. Smell and taſte,

When diſtilled with water, it imparts ſome of its taſte to that fluid. It is perfectly ſoluble in alcohol, the ſolution becoming turbid when water is added. imparted to water. Solution in alcohol.

Sulphuric acid decompoſes it in part, when the mixture is heated. Nitric acid acts on it as on the eſſential oils in general, converting it into a reſinous ſubſtance. It is not affected by muriatic acid, but is partially ſoluble in acetic acid. It ſinks in water; and forms ſaponaceous compounds with the alcalies. Action of ſulphuric acid. Of nitric acid. Of muriatic acid, and the alcalies.

Such are the general properties of the acid, and oil of benzoin, much more might undoubtedly be ſaid concerning them; but as the foregoing experiments, were not made with a view of inveſtigating their properties, it would be unneceſſary to give a long account of them in this place. It muſt be obſerved that the pureſt benzoin has been made uſe of throughout; and laſtly, that no traces of alcali, were found in the coal which remains after diſtillation.

Arlington-Street.

IV.

On the Devitrification of Glaſs and the Phenomena which happen during its Cryſtallization. By DARTIGUES.

(Concluded from Page 64.)

WHEN clear glaſſes contain a certain quantity of a neutral ſalt which the fire has not had time or force to diſſipate, it often happens, during ſlow cooling, that the appearances called greaſe, threads, bubbles, and ſpecks, are ſpontaneouſly and ſuddenly formed. Imperfect glaſſes.

* This table of affinities was formed according to the experiments of Tromſdorff. Vide Thomſon's Syſtems of Chemiſtry, Vol. II. page 126. 2nd edition.

These

These accidents, their different caufes and remedies, are treated at length in the firft part of my work, where I fpeak of the fonding of glafs; but though the explanation of thefe facts entirely belongs to the theory of devitrification, it will be fufficient in this place to mention the phenomena which arife from the prefence of the different earths.

Cryftallization of lime in glafs, Colourlefs glaffes contain more or lefs of lime, for the reafons which I have mentioned in fpeaking of the different compofitions of glafs. This lime, when in excefs, gives cryftals very well, as Loyfel remarks: they are eafily known, and fo abundant, that they abfolutely impair the tranfparency: they are prifms, which feem to float in the midft of the pafte of glafs, and tend to unite in ftars differently figured: thefe prifms are all nearly of the fame thicknefs, and two or three millimetres in length (about one-tenth of an inch).

in large maffes. When this cryftallization takes place fpontaneoufly upon large maffes at the bottom of the furnace, the colour of the glafs becomes darker, and inclines to black, by the mixture of a certain quantity of the afhes. The ftriated ftars here fpoken of, become more numerous, the more remote they are from the fide in contact with the fire. Cryftallizations at firft infulated, are foon fucceeded by a mafs entirely cryftallized, in which the vitreous character is no longer diftinguifhable.

Modified cryftallizations in glafs. Thefe are the moft common characters of cryftallization; but others are frequently feen, which are certainly owing to accident, and well deferve notice on account of their varieties*. I have fome pieces of glafs in which cryftals are difcernable, fo fine, that they can fcarcely be feen with a magnifier. They are prifms divergent from the fame center, and forming ftars, which frequently do not exceed one millimetre (or half a tenth of an inch) in diameter: their union refembles a flight mift in the pafte of the glafs.

Some pieces have the afpect of a faline cruft applied to a foreign body, againft which the glafs was in contact. In fome of thefe this cruft being compofed of ftriated protuberances, appears to advance more and more into the glafs itfelf.

Laftly, there is another variety perhaps more curious than all the reft, in which centers, or cryftallizations refembling

* Mr. Sage poffeffes a piece of glafs cryftallized in fix-fided bafaltic prifms, totally devitrified.

peas,

peas, and almoſt ſimilar to grain, are ſeen in the midſt of the paſte of glaſs. They are ſmall ſpheres flattened at each end with an indentation in the middle of each depreſſion. The ſides have facetts, like the grain of Indian naſturtium, and theſe faſſetts are always ſix in number.

I intend, when I ſhall have procured a ſufficient number of this ſingular ſpecies of cryſtal, to analyze it, in order to determine which of the earths it is that effects ſo extraordinary a form.

I have thus given a ſhort account of the principal facts which characterize the precipitation and cryſtallization of glaſs. It is evident that they are of the ſame nature as thoſe produced by the cementation pointed out by Reaumur; and that this cementation is always a more or leſs abſolute devitrification of the glaſs.

When the glaſs is devitrified, it has no longer a vitreous but a granulated fracture; it has no tranſparency, and perfectly reſembles a ſtone; it becomes a leſs bad conductor of heat and of electricity; laſtly, it is no longer ſuſceptible of fuſion at the ſame degree of heat; and in order to reſtore it more eaſily to the vitreous ſtate, it muſt firſt be pulverized, to bring into contact thoſe ſubſtances which, during the cryſtallization, had become ſeparated from each other, and could no longer ſerve as fluxes to each other.

Recapitulation.

Revitrification.

I hope the novelty of many of the facts I have here indicated, and the conſequences I have drawn from them, may be ſufficiently intereſting to philoſophers to have ſome claim to their attention. I have no doubt but that other general inductions will preſent themſelves to them which may have eſcaped me, or which could not be introduced in a ſhort memoir. The reſemblance of my ſpecimens of devitrified glaſs with thoſe of certain lavas; the poſſibility that other lavas may have undergone a more complete devitrification by a much ſlower cooling beneath the volcanic currents which are known to have flowed, or to have remained fluid for whole years beneath congealed cruſts;—every fact leads me to think, that theſe phenomena may afford a ſolution of many geological events, concerning which no general opinion has yet been adopted, becauſe there was no reaſon to believe that ſtones may have formerly poſſeſſed the vitreous ſtate.

General concluſion and application to geology.

Letter

V.

Letter from a Correſpondent, relating to the apparent Reflection of Cold and the Invention of the Teleſcope, as noticed by Sir H. C. ENGLEFIELD *in laſt Month's Journal.*

To Mr. NICHOLSON.

SIR,

IN juſtice to Profeſſor Pictet, I truſt you will have the goodneſs to inſert in your next number the words of a note in his *Eſſai de Phyſique*, p. 81, where he deſcribes his experiment on the apparent reflection of cold :

Apparent reflection of cold.

" *Les Académiciens* del Cimento *avoient eſſayé de concentrer le froid au foyer d'un miroir concave; mais ils avouent eux-mêmes, que leur expérience avoit été faite d'un manière trop inexacte pour qu'on pût en rien conclure de certain.*"

Perhaps Profeſſor Pictet ſpeaks too lightly of the experiment of the Academicians; but ſo far was that experiment from being forgotten, that the account of it has been very lately reprinted in the firſt volume of the Journals of the Royal Inſtitution, p. 224; a work to which your correſpondent, who has revived the ſubject (page 1 of your laſt Number), has himſelf contributed ſome valuable papers.

Kepler did not invent or know of the teleſcope before Galileo.

With reſpect to the invention of the teleſcope, give me leave to obſerve, that *perſpicillum* means ſimply the ſight of an aſtronomical inſtrument, having a ſmall perforation. If the comet in queſtion was ſurrounded by a ſmall round and well-defined nebuloſity, ſuch a ſight might eaſily produce the effect attributed to it. A teleſcopic ſight is uſually called *perſpicillum vitreum.* Galileo ſays, in a work publiſhed in 1610, that he was informed of the invention of teleſcopes about nine months before. Kepler wrote, in 1611, a valuable work on the theory of dioptrics, in which the conſtruction of the aſtronomical teleſcope is deſcribed. Had he been the original inventor of the Galilean teleſcope, he would not omitted this opportunity of ſubſtantiating his claim to ſo important a diſcovery.

I am, Sir,

Your very obedient ſervant,

ALETES.

Facts

VI.

Facts tending to decide the Question, at what Point of Temperature Water possesses the greatest Density. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

ACCORDING to the doctrine in most of our books treating of the elements of chemical science, it is considered as a decided fact, that water is of the greatest density at or near 40° of temperature of Fahrenheit's scale; and that, above or below that temperature, it expands alike by heat or cold for a certain number of degrees. I have taught a different doctrine for some time past; which is, that water is densest at 32°, or the freezing point, and that it expands alike above and below that point for at least 25° (provided it does not freeze), and that the quantity of expansion is as the square of the temperature, reckoned from 32°, up or down; that is, if the expansion from 32 to 42° be denoted by 1, that from 32 to 52° will be denoted by 4, and that from 32 to 62° by 9, *&c.* or nearly so; the deviation from strict accuracy being, as I conceive, occasioned by the mercurial thermometer not being an exact measure of temperature: the expansion at 22° is likewise 1, at 12° it is 4, and at 2° it is 9, *&c.*; the same below as above the freezing point. In consequence of this, a gentleman of professional eminence has undertaken to examine the subject anew; and has been led, by an ingenious train of experiments totally different from the following, to decide in favour of the common notion, that water is densest about 40°. These experiments of his will soon be published. I am still, however, decidedly convinced, that my opinion above stated is true, and that principally from the facts I am about to state, which those who adopt the common opinion must see the absolute necessity of explaining upon their principle, or otherwise, of controverting the facts themselves. As this season of the year is most convenient for experiments of this nature, I am the more solicitous to have the facts announced, leaving all explanation for the present. They are of a very simple nature, and may be investigated with little trouble and expence.

General notion that water is densest at 40°, &c.

The author's doctrine that 32° is the point of greatest density.

Experiments brought to support the former conclusion.

The author retains his opinion, and invites other experiments.

A number

Inftructions for experiments. Procure large bulbed thermometers, of earthen ware, glafs, metal, &c.

A number of water thermometers are to be procured, the containing veffels of which are of different materials, as earthen ware, glafs, and various metals. Each of thefe fhould contain one or two ounces (from 4 to 800 grains), more or lefs, of water. Common brown inkftands, which go by the name of Nottingham ware, anfwer very well for one fpecies, but they require to be well painted without, as they are not otherwife water-tight. I have a few of Queen's ware, made purpofely in Staffordfhire, which conftitute another fpecies of earthen ware; fome of them are glazed in and out; others unglazed, but thefe being painted without are made water-tight, and expand the fame by heat as glazed ones: Of glafs, common thermometer tubes, with larger bulbs than ordinary, are fufficient. I have the metallic veffels made in the fhape of cylindrical tin cannifters, conical towards the top, and at the fummit a fmall cylindrical tube, fuch as to take a thermometer tube. The glazed earthen ware and the metallic, require moftly to be painted before they are quite tight.

Fill them with boiled water.

The veffels being thus prepared, they are to be filled with water previoufly boiled to expel the air; a thermometer tube with cement is then fuddenly plunged into the veffel and cemented faft; the water may then be driven out of the tube by heat, or more may be put into it by a fmall wire; it is then fit for ufe, and a fcale of equal parts may be applied to the tube; or it may be divided, and marks made with a file or paint.

Some of the refults of my experiments with inftruments of this kind, are as follow:

Experimental refults noted with three earthen, one glafs, two iron, one copper, one brafs, one pewter, and one leaden thermometer.

	Water loweft.	Water the fame height.
1. Brown earthen ware, No. 1, at	36°	at 32° and 40°
2. Brown earthen ware, No. 2,	38	32 and 44
3. Queen's ware,	40	32 and 48
4. Flint-glafs,	$41\frac{1}{2}$	32 and 51
5. Iron, thin plate,	$42\frac{1}{2}$	32 and 53
6. Tinned iron,	$42\frac{1}{2}$	32 and 53
7. Copper,	$45\frac{1}{2}$	32 and 59
8. Brafs,	46	32 and 60
9. Pewter,	46	32 and 60
10. Lead,	$49\frac{1}{2}$	32 and 67

Another

Another phenomenon in these instruments is observable; it is not new, but it deserves a marked attention in the present enquiry: If the apparent expansion of water for the first 10° of temperature, reckoned from the lowest point in any of the above instruments, be denoted by 1; then if the instrument, taken at any temperature, be suddenly plunged in water of 10° higher temperature, the water instantly *sinks* a considerable way, occasioned no doubt by the vessel being extended by the heat before the water it contains has time to expand. The quantity of depression in the different instruments was found as under:

Phenomenon of the depression of a thermometrical fluid when the instrument is suddenly heated.

Table of this depression in the different thermometers.

Brown earthen ware sinks by being dipped in water of 10° higher temperature,	.2
Queen's ware,	.3
Flint-glass,	.25
Iron,	.66
Copper,	.9
Brass,	1.1
Pewter,	1.0
Lead,	1.5

I submit these facts to the consideration of those who feel interested in the enquiry; and desire that they may be particularly investigated by those who maintain that water is densest at 40°.

I remain your's,

J. DALTON.

Manchester, Jan. 10, 1804.

VII.

Analytical Experiments and Observations on Lac. By CHARLES HATCHETT, *Esq. F. R. S. From the Philosophical Transactions for* 1804.

(Concluded from p. 54.)

Properties of the colouring Extract of Lac.

1. WHEN dry, it is of a deep red colour, approaching to purplish crimson.

Colouring extract of lac.

2. Being put on a red-hot iron, it emits much smoke, with a smell somewhat resembling burned animal matter, and leaves a very bulky and porous coal.

3. Water,

Colouring extract of lac.

3. Water, when digefted with it in a boiling heat, partially diffolves it; but the refiduum was found to be abfolutely infoluble in water.

4. Alcohol acts but flowly on it; and, in a digefting heat, diffolves lefs than water. The colour of the folution is alfo not fo beautiful; and a confiderable part of the refiduum left by alcohol was, when digefted with water, found to be foluble, although this was not the cafe, when the refiduum left by water was treated with alcohol.

5. It is infoluble in fulphuric ether, excepting a very fmall portion of refin, which appeared to be accidentally mixed with it.

6. Sulphuric acid readily diffolves it, and forms a deep brownifh-red folution, which, being diluted with water, and faturated with potafh, foda, or ammonia, becomes changed to a deep reddifh-purple.

7. Muriatic acid diffolves only a part: the folution is of the colour of port-wine, and, by the alkalis, is changed to a deep reddifh-purple.

8. Nitric acid fpeedily diffolves it: the folution is yellow, and rather turbid; but the red colour is not reftored by the alkalis, for thefe only deepen the yellow colour. This nitric folution did not afford any trace of oxalic acid.

9. Acetic acid diffolves it with great eafe, and forms a deep brownifh-red folution.

10. Acetous acid does not diffolve it quite fo readily, but the folution is of a brighter red. Both of the above, when faturated with alkalis, are changed to a deep reddifh-purple.

11. The lixivia of potafh, foda, and ammonia, act powerfully on this fubftance, and almoft immediately form perfect folutions, of a beautiful deep purple colour.

12. Pure alumina, put into the aqueous folution, does not immediately produce any effect; but, upon the addition of a few drops of muriatic acid, the colouring matter fpeedily combines with the alumina, and a beautiful lake is formed.

13. Muriate of tin produces a fine crimfon precipitate, when added to the aqueous folution.

14. A fimilar coloured precipitate is alfo formed, by the addition of folution of ifinglafs.

2 Thefe

Thefe properties of the colouring fubftance of lac, efpecially its partial folubility in water and in alcohol, and its infolubility in ether, together with the precipitates formed by alumina and muriate of tin, indicate that this fubftance is vegetable extract, perhaps flightly animalized by the coccus.

The effects which it produced on gelatin, alfo demonftrate the prefence of tannin; but this very probably was afforded by the fmall portions of vegetable bodies, from which the ftick lac can feldom be completely feparated.

Properties of the Refin of Lac.

This fubftance is of a brownifh-yellow colour; and, when put on a red-hot iron, it emits much fmoke, with a peculiar fweet odour, and leaves a fpongy coal. Refin of lac.

It is completely foluble in alcohol, ether, acetic acid, nitric acid, and the lixivia of potafh and foda.

Water precipitates it from alcohol, ether, acetic acid, and partially from nitric acid; and it poffeffes the other general characters of a true refin.

Properties of the Gluten of Lac.

It has been already obferved, that when fmall pieces of fhell lac have been repeatedly digefted in cold alcohol, they become white, bulky, and elaftic. By drying, thefe pieces become brownifh and brittle; the elafticity is alfo deftroyed by boiling water, exactly as when the gluten of wheat is thus treated. Gluten of lac.

If the pieces of fhell lac, after the digeftion in alcohol, be digefted with diluted muriatic acid, or with acetic acid, the greater part of the gluten is diffolved, and may be precipitated, in a white flaky ftate, by alkalis; but, if thefe laft be added to excefs, and heat be applied, then the glutinous fubftance is rediffolved, and may be precipitated by acids.

If the pieces of fhell lac, after digeftion in alcohol, be treated with alkaline lixivia, then the whole is diffolved, and forms a turbid folution. But, when acids are employed, the chief part of the gluten is alone acted upon, and a confiderable refiduum is left, confifting of the wax, fome

of the refin, and a portion of gluten, which has been protected from the action of the acid by the two former fubftances.

The above properties indicate a great refemblance between this fubftance and the gluten of wheat; I therefore have called it gluten, but, at a future time, I intend to fubject it to a more accurate examination.

Properties of the Wax of Lac.

Wax of lac.

If fhell lac be long, and repeatedly digefted in boiling nitric acid, the whole is diffolved, excepting the wax, which floats on the furface of the liquor, like oil, and, when cold, may be collected; or it may be more eafily obtained in a pure ftate, by digefting the refiduum left by alcohol in boiling nitric acid.

The wax thus obtained, when pure, is pale yellowifh white, and (unlike bees wax) is devoid of tenacity, and is extremely brittle.

It melts at a much lower temperature than that of boiling water, burns with a bright flame, and emits an odour fomewhat refembling that of fpermaceti.

Water does not act upon it, neither does cold alcohol; but this laft, when boiled, partially diffolves it, and, upon cooling, depofits the greater part; a fmall portion, however, remains in folution, and may be precipitated by water.

Sulphuric ether, when heated, alfo diffolves it; but, upon cooling, nearly the whole is depofited.

Lixivium of potafh, when boiled with the wax, forms a milky folution; but the chief part of the wax floats on the furface, in the ftate of white flocculi, and appears to be converted into a foap of difficult folubility; it is no longer inflammable, and, with water, forms a turbid folution, from which, as well as from the folution in potafh, the wax may be precipitated by acids.

Ammonia, when heated, alfo diffolves a fmall portion of the wax, and forms a folution very fimilar to the former.

Nitric and muriatic acids do not feem to act upon the wax; the effects of fulphuric acid have not been examined.

When the properties of this fubftance are compared with thofe of bees-wax, a difference will be perceived; and, on the contrary,

contrary, the moſt ſtriking analogy is evident, between the wax of lac and the myrtle wax which is obtained from the Myrica cerifera.

It greatly reſembles myrtle wax.

An account of the latter ſubſtance has been publiſhed by Dr. Boſtock, of Liverpool, in Nicholſon's Journal, with comparative Experiments and Obſervations on bees-wax, ſpermaceti, adipocire, and the cryſtalline matter of biliary calculi.*

The properties of the myrtle wax, as deſcribed in Dr. Boſtock's valuable paper, ſo perfectly coincide with thoſe which I have obſerved in the wax of lac, that I cannot but conſider them as almoſt the ſame ſubſtance; indeed I think they may be regarded as abſolutely identical, if ſome allowance be made for the ſlight modifications which have been produced by the different mode of their formation.

From the preceding experiments and analyſes we find, that the varieties of lac conſiſt of the four ſubſtances which have been deſcribed, namely, extractive colouring matter, reſin, gluten, and a peculiar kind of wax. Reſin is the predominant ſubſtance; but this, as well as the other ingredients, is liable, in a certain degree, to variation in reſpect to quantity.

Component parts of lac.

According to the analyſes which have been deſcribed, one hundred parts of each variety of lac yielded as follows:

Stick Lac.

Reſin - - - -	68.
Colouring extract - - -	10.
Wax - - - -	6.
Gluten - - - -	5.50
Extraneous ſubſtances - -	6.50
	9.60.

Seed Lac.

Reſin - - - -	88.50
Colouring extract - - -	2.50
Wax - - - -	4.50
Gluten - - - -	2.
	97.50.

* Nicholſon's Journal for March, 1803, p. 129.

Shell Lac.

Refin - - - -	90.90
Colouring extract - - -	0.50
Wax - - - -	4.
Gluten - - - -	2.80
	98.20.

The proportions of the fubftances which compofe the varieties of lac, muft however be fubject to very confiderable variations; and we ought therefore only to confider thefe analyfes in a general point of view. Hence we fhould ftate, that lac confifts principally of refin, mixed with certain proportions of a peculiar kind of wax, of gluten, and of colouring extract.

The relative quantity of the two latter ingredients, very confiderably effect the characters of the lacs; for inftance, we may obferve, that the glutinous fubftance, when prefent in fhell lac in a more than ufual proportion, probably produces the defect obferved in fome kinds of fealing wax, which, when heated and burned, become blackened by particles of coal; for the gluten affords much of this fubftance, and does not melt, like the refin and wax. From what has been ftated, therefore, lac may be denominated a *cero-refin*, mixed with gluten and colouring extract.

§ III.

GENERAL REMARKS.

Lac has the characters of vegetables.

From the whole of the experiments which have been related, it appears that although lac is indifputably the production of infects, yet it poffeffes few of the characters of animal fubftances: and that the greater part of its aggregate properties, as well as of its component ingredients, are fuch as more immediately appertain to vegetable bodies.

It is very ufeful;

Lac, or gum lac, as it is popularly but improperly called, is certainly a very ufeful fubftance; and the natives of India furnifh full proofs of this, by the many purpofes to which they apply it.

for rings, beads;

According to Mr. Kerr, it is made by them into rings, beads, and other female ornaments.

When

When formed into sealing wax, it is employed as a japan, and is likewise manufactured into different coloured varnishes. sealing wax, varnishes;

The colouring part is formed into lakes for painters: a sort of Spanish wool for the ladies is also prepared with it: and, as a dying material, it is in very general use. lakes, dying;

The resinous part is even employed to form grindstones, by melting it, and mixing with it about three parts of sand. For making polishing grindstones, the sand is sifted through fine muslin; but those which are employed by the lapidaries, are formed with powder of corundum, called by them Corune.* grindstones, and polishers;

But, in addition to all the above uses to which it is applied in India, as well as to those which cause it to be in request in Europe, Mr. Wilkin's Hindû ink occupies a conspicuous place, not merely on account of its use as an ink, but because it teaches us to prepare an aqueous solution of lac, which probably will be found of very extensive utility. the Hindû ink;

This solution of lac in water may be advantageously employed as a sort of varnish, which is equal in durability, and other qualities, to those prepared with alcohol; whilst, by the saving of this liquid, it is infinitely cheaper. water varnish:

I do not mean however to assert that it will answer equally well in all cases, but only that it may be employed in many. It will be found likewise of great use as a vehicle for colours; for, when dry, it is not easily affected by damp, or even by water.

With a solution of this kind, I have mixed various colours, such as vermillion, fine lake, indigo, Prussian blue, sap green, and gamboge; and it is remarkable, that although the two last are of a gummy nature, and the others had been previously mixed with gum, (being cakes of the patent water-colours,) yet, when dried upon paper, they could not be removed with a moistened sponge, until the surface of the paper itself was rubbed off. The latter very useful. Instances.

In many arts and manufactures, therefore, the solutions of lac may be found of much utility; for, like mucilage, they may be diluted with water, and yet, when dry, are little if at all affected by it.†

We

* Phil. Trans. 1781, p. 380.

† The alkaline solutions of lac are evidently of a saponaceous nature, and, like other soaps, may be decomposed by acids. The entire

Refins acted on by acids and alkalis.

We find, from the experiments on lac, that this fubftance is foluble in the alkalis, and in fome of the acids. But this fact (confidering that refin is the principal ingredient of lac) is in oppofition to the generally received opinion of chemifts, namely, that acids and alkalis do not act upon refinous bodies. Some experiments, however, which I have made on various refins, gum-refins, and balfams, fully eftablifh, that thefe fubftances are powerfully acted upon by the alkalis, and by fome of the acids, fo as to be completely diffolved, and rendered foluble in water.

Field of inquiry.

It will be a very wide and curious field of inquiry, to difcover what changes are thus produced in thefe bodies, efpecially by nitric acid. Each fubftance muft form the fubject of a feparate inveftigation; and there cannot be a doubt but that much will be learned refpecting their nature and properties, which hitherto have been fo little examined by chemifts.

Utility of refinous folutions.

The alkaline folutions of refin may be found ufeful in fome of the arts; for many colours, efpecially thofe which are metallic, when diffolved in acids, may be precipitated, combined with refin, by adding the former to the alkaline folutions of the latter. I have made fome experiments of this kind with fuccefs; and perhaps thefe proceffes might prove ufeful to dyers and manufacturers of colours. It is probable alfo, that medicine may derive advantages from fome of this extenfive feries of alkaline and acid folutions of the refinous fubftances.

entire fubftance of lac is not however completely diffolved, as appears from the turbidnefs of the liquors. Three of the four ingredients, namely, the refin, the gluten, and the colouring extract, appear to be in perfect folution; whilft the wax is only partially combined with the alkali, and forms that imperfectly foluble faponaceous compound which has been formerly mentioned, and which remains fufpended, and difturbs the tranfparency of the folution.

From various circumftances, it does not feem improbable, that the long fought for, but hitherto undifcovered vehicle employed by the celebrated painters of the Venetian School, may have been fome kind of refinous folution, prepared by means of borax, or by the alkalis.

A fimple

VIII.

A simple and accurate Method of Surveying on Shore, with such Instruments only as are in every one's Possession. By Captain J. MORTLOCK. *From the Author.*

To Mr. NICHOLSON.

28, Surry Place, Kent Road,
SIR, *7th January,* 1805.

ALMOST all our treatises on nautical surveying begin with the explanation and description of what are called the necessary instruments, which are described so numerous, and the price so considerable, that very few can procure them: Thus discouraged, they abandon every idea of making plans of such ports as they touch at, for want of what they conceive to be the necessary instruments. Introductory letter.

To obviate this difficulty, and to render nautical surveying more general, I have, in the annexed paper, attempted to shew the mariner how to survey any port or place he may touch at, with great accuracy, little trouble, and without any expence for instruments. Should you find this simple method deserving of a place in your valuable Journal, I shall feel myself honoured by your inserting it.

I am, Sir,

Your most obedient humble servant,

J. MORTLOCK.

FIRST make an eye-sketch of the place to be surveyed, as the annexed figure, numbering all the points, bays, rocks, shoals, &c. Choose two stations, as A and B *(Fig. 1, Plate V)*, whence all the rocks, points, &c. may be seen from, and so situated from each other, that the bearings of the points, &c. as taken from A and B, shall intersect at angles at least greater than 10 degrees, but the nearer 90 degrees the better. Nautical surveying without instruments.

Having chosen the stations, proceed to one of them as A, and place the paper intended to receive the plan horizontally before you, extended by pins, or otherwise, on a board securely fixed, to prevent it shifting its position while the bearings are drawing.

Stick

Nautical furveying without inftruments.

Stick a pin through the paper firm into the board, at the part meant to reprefent the ftation A, and lay a ruler with a perfect ftraight edge on the paper, touching the pin at A and pointing towards the ftation B, and draw the line A B: in like manner draw lines from A towards all the points, rocks, bays, &c. numbering the lines as the points, rocks, bays, &c. are numbered in the eye-fketch: Proceed next to the ftation B, and place the board horizontally before you, fo that the line A B fhall point back towards A, and fecure the board with the fame precaution as at A, to prevent its fhifting: then, in the line A B, ftick a pin firm through the paper into the board, in that part meant to reprefent the ftation B; from which point draw lines pointing towards the different points, rocks, &c. as was done from A, numbering them in like manner. Now, where the lines drawn from B interfect thofe of the fame number drawn from A, will be the place of the points, rocks, &c. to which the lines were directed to from the ftations. Sketch in the fhore between the points, &c. and the plan is completed.

The meridian-line may be found by compafs, or more correctly, by drawing the line of the fun's bearing from one of the ftations, and taking his altitude at the fame time. Then with the latitude, altitude, and declination, compute the azimuth, and lay it off to the left or right of the line of the fun's bearing, according as the fun was to the right or left of the meridian, and it will give the true north and fouth, or meridian-line.

If the diftance between any two points on the fhore be meafured, it will give you a fcale for the plan; but it may often be found more convenient to meafure off a bafe, as A C, from one of the ftations, in a direction nearly perpendicular to the line A B; and let it be in length equal to fome part of a geographical mile, as 380 feet $= \frac{1}{16}$, or 760 $= \frac{1}{8}$, or 1520 $= \frac{1}{4}$, or 3040 $= \frac{1}{2}$, or any part of a mile; then will the line A C be a fcale to the plan.

I have fuppofed any common board and ruler to illuftrate the fimplicity of this method of furveying: but to fuch as are provided with a drawing frame, it will be found convenient to extend the paper on; and if a ruler has fights perpendicular to its edge, it will be found commodious, and require lefs trouble. I hope the eafe and expedition with which the whole

is

is performed, will induce ſea-faring people to amuſe themſelves by taking plans of the places they touch at: for it is by the improvement of geography that the dangers of navigation are diminiſhed, and, conſequently, the lives and property embarked in our ſhipping are leſs expoſed to danger.

J. MORTLOCK.

IX.

Notice of an Omiſſion in ACCUM'S *Chemiſtry, of the direct Production of Nitric Acid. By* W. F. S.

To Mr. NICHOLSON.

SIR, *Lincoln's Inn, January* 7, 1805.

Nitre and nitric acid in Accum's chemiſtry.

HAVING in your Philoſophical Journal ſpoken very handſomely, and not undeſervingly ſo, of Mr. Accum's Treatiſe on Chemiſtry, it would not perhaps be improper in one of your enſuing Numbers, either to elucidate or correct a ſmall difficulty, or rather error, in that book. In deſcribing the mode of producing nitric acid, in the ſecond volume of Mr. A.'s work, p. 286, he puts as a principal ingredient *nitrate of potaſh:* Now, on referring to the mode of obtaining *nitrate of potaſh* in page 287, and following page, it is deſcribed as being produced by neutralizing the carbonate of potaſh with *nitric acid:* Now, Sir, it is pretty evident, that the means of preparing either nitric acid or nitrate of potaſh is not given, or rather it is ſtated by *implication*, as *incapable of being produced by art.*

Very reſpectfully your's,

W. F. C.

P. S. As this error can only be rectified by a ſubſequent edition, it would, with ſubmiſſion, be extremely beneficial to the holders of the preſent edition, that you ſhould introduce the emendation in your Journal.

On

X.

On Galvanifm. By Mr. Charles Sylvester.

To Mr. NICHOLSON.

SIR,

Difficulties refpecting the galvanic decompofition of water; as to the diftances of the wires.

YOU did me the honour of inferting in your valuable Journal, fome experiments tending to illuftrate the theory of galvanifm. I do not know who originally propofed the idea of the combination of electricity with hydrogen (the truth of which my experiments were intended to eftablifh), though I am now bound to acknowledge the ingenuity and importance of the thought. Such an idea would perhaps never have been fuggefted, had it not been for the very paradoxical appearances attending the decompofition of water by galvanifm. The appearance of the hydrogen at fo great a diftance from the oxigen, both of which muft have been produced from the fame particle of water, was very fatisfactorily accounted for by this conjecture. The continental philofophers, and Dr, Gibbes of Bath, did not fo well account for the phēnomena by their hypothefis, though it appeared fo formidable, as to threaten the theory of modern chemiftry with diffolution.

Hypothefis of Mr. Wilkinfon cenfured.

Another hypothefis of the decompofition of water, was given by Mr. Wilkinfon, in his Elements of Galvanifm, and in your 36th Number. If we even allow Mr. W. the advantage of all his very gratuitous data in accounting for the decompofition of water, the contradiction with which they abound, will totally render his hypothefis invalid.

Remarks in detail on Mr. W.'s hypothefis.

Mr. W. begins by fuppofing a particle of water analogous to the Leyden phial, which is the fame thing as to fuppofe that water, a conductor of electricity, is compofed of particles in themfelves non-conductors. Mr. W. is of opinion, that the decompofition takes place in the middle of the liquid between the points of the wires. The way in which he fuppofes the feparation to be effected, is fomething like the idea the ancients had of the folutions of metals in acids, *viz.* that the metal was fplit into very minute particles (capable of being fufpended in the liquid) by the wedge-like particles of the acid. After the decompofition is effected, he tells us that the capacity of the hydrogen is diminifhed for electricity, while

while the capacity of oxigen is increafed for the fame fubftance; two facts for which I fhould be glad to know Mr. W.'s authority. He now fuppofes the oxigen to be attracted by the pofitive wire, where it faturates itfelf with the electricity neceffary to conftitute its gafeous form. The hydrogen, on the other hand, is attracted to the negative wire, to which it muft give its excefs before its elaftic form can be effected.

Objection.

After all the labour Mr. W. has beftowed to bring the hydrogen to one wire, and the oxigen to the other, he does not appear, in my opinion, to have fucceeded; for the excefs of electricity in the hydrogen would be given to the deficient oxigen, and the gaffes would be given out in the middle of the liquid, and not at the ends of the wires.

Water not decompofed in a very fmall tube.

Mr. W. mentions the curious fact of water not being decompofed in a very fmall tube, and alfo, that no decompofition takes place when the wires in the liquid are as much as eight inches diftant from each other. I have long been in poffeffion of thefe facts: I have varied thefe experiments by ufing, inftead of pure water, different folutions of falts, with a view to increafe the conducting power of the liquid medium: I found a folution of carbonate of potafh to anfwer very well: I placed the wires in the ends of a tube of more than a yard in length: The decompofition went on very rapidly: I foon obferved the pofitive wire coated with beautiful carbonate of copper, at the fame time I obferved a gafeous fubftance difengaged: I found it, by the teft of lime-water, to be carbonic acid gas. I obferved the difengagement of this gas, after I had rendered the potafh very cauftic by lime.

Saline folutions are decompofed.

Carbonate of potafh.

Pure potafh.

Common falt.

I afterwards ufed a tube of $\frac{1}{20}$th of an inch in diameter, and five feet long, into which I introduced a faturated folution of common falt. After the wires were introduced, and the communication made, I obferved bubbles of hydrogen upon the negative wire, in about a minute after *.

I remain, Sir,

Your's, &c.

CHARLES SYLVESTER.

Sheffield, Jan. 20, 1805.

* This corroborates the fact of the hydrogen paffing invifibly through the liquid.

XI.

Reply to the Animadverſions and Experiments of C. L. *on the Subject of the Horizontal Moon. By* Mr. Ezekiel Walker.

To Mr. NICHOLSON.

Dear Sir,

WHAT can have induced your correſpondent C. L. to attempt to confute my paper relating to the horizontal moon, with ſo much invidiouſneſs, is beſt known to himſelf. For my own part, I believe that he is perſonally unknown to me, and that I am equally ſo to him.

Whether the teleſcope be properly urged againſt Mr. W.'s theory.

The firſt ſtep that this faſtidious writer takes to diſprove the truth of my poſition is a falſe one. He ſays, that " if Mr. Walker's poſition were true, the magnifying power of the ſame teleſcope would vary with its aperture." *

C. L. has been very unfortunate in mentioning the teleſcope, for the properties of that inſtrument will confute every argument which he has uſed againſt my theory, and ſhow that his ill conducted experiments, like an *ignis fatuus*, tended only to miſlead him.

The want of a ſtandard would prevent its being ſeen whether a leſs light gives a greater appearance to objects.

I ſhall drop this ſubject for the preſent, to examine his next aſſertion in the ſame paragraph. The apparent magnitude of the paper before the eye may become larger, when the candle is ſhaded with the hand, for any thing that C. L. knows to the contrary, as the increaſe is too ſmall to be perceived by our ſenſes; and even if it were ten times larger than it is, it could not be known, becauſe every other object before the eye would increaſe in apparent magnitude at the ſame time, and in the ſame ratio; and conſequently leave no ſtandard to compare the paper with. To elucidate this in a familiar way, permit me, Sir, to aſk how would C. L. determine the number of miles between Troſton in Suffolk, and Soho-Square in London, without ſome ſtandard meaſure, with which to compare that diſtance.

Strictures on C. L.'s experiments.

Then follow his " correct experiments of the ſame deſcription as mine."—My experiments were made to imitate the eye, upon a large ſcale. Let us ſee how C. L. has imitated nature in his experiments.

* Philoſ. Journal, Vol. IX. p. 235.

In

In his firft experiment, "from the flame to the lens the diftance was $49\frac{1}{2}$ inches, and from the lens to the image it was 88 inches." Thefe numbers being reduced to the fcale of the human eye, it will appear that the diftance between the cryftalline lens and the retina, is nearly twice as great as between the cryftalline lens and the object; confequently, if we take the diftance between the cryftalline and the outward furface of the cornea into the calculation, the object muft nearly touch the eye. This, I think, may be called *a fhort-fighted experiment*.

Exp. 1. The object was too near the lens.

The conclufion of C. L.'s paper contains a number of particulars, in which he *fuppofes* I have erred, and then modeftly "fubmits it to yourfelf and readers, whether I have acted confiftently with the rules of philofophical inveftigation," &c.

But as it will appear hereafter, that none of thofe errors had any exiftence, except in C. L.'s own mind, his conclufion can ferve no other purpofe than to fhew the temper and difpofition of the writer.

In my paper which was honoured with a place in the 9th vol. of this Journal, page 164, I only gave an abftract of a feries of experiments, on which I founded my general conclufion; but it now appears neceffary to give a more particular account of them, to fhew that they are entitled to more credit than C. L. has thought fit to give them.

A more particular account of the author's former experiments.

After having prepared my apparatus as defcribed in my paper above-mentioned, with a long mould candle of fix to the pound, placed in an inclined pofition, I began with meafuring the focal image of the whole lens.

The length of this luminous picture upon the fcreen, was determined by a pair of compaffes, and a diagonal fcale of inches. This meafure was entered on page 1 of a book, prepared for that purpofe.

The card No. 1. was then placed before the lens; the luminous picture meafured in the fame manner as before, was entered on page 2 of the fame book.

The meafure of the picture given by the aperture, No. 2. was entered on the 3d page. And the meafure of that given by No. 3. was noted down on the 4th page.

As foon as thefe four experiments were finifhed, I began again with the whole lens, and entered a fecond meafure on page 1. under the firft, and continued till I had obtained two meafures

meaſures on each page: and in this manner, I continued my experiments until I had obtained five or ſix meaſures to each aperture. This mode gave me an opportunity of ſeeing how much the flame of the candle altered in its length, during the time that I was making the experiments, which alteration was too ſmall to be regarded. The meaſures on each page being added up, and divided by the number of them, gave me thoſe numbers which were inſerted in my paper.

But I did not ſtop here, for ſets of experiments were repeated in the ſame manner, on ſeveral evenings, to aſcertain the fact more clearly. But to come more immediately to the point; when the ſun or moon was uſed inſtead of a candle, the ſame reſult was obtained, viz. the greateſt aperture gave the greateſt luminous picture.

Principles of optics brought in ſupport of the author's theory.

The truth of this property in optics, however, does not wholly reſt on my experiments, the ſame concluſion may be derived from other principles.

The pencils of light that enter the eye are ſmaller, the ſmaller the aperture of the object lens;

It is a well known property of the teleſcope, that "as the aperture is contracted, the ſlender pencils or cylinders of rays that emerge from the eye-glaſs into the eye, are alſo contracted in the ſame proportion." *

The magnifying power, of the object glaſs of a teleſcope is not increaſed by increaſing its aperture; but by increaſing its aperture, you increaſe the magnitude of the pencil of light in its focus: for as the eye-glaſs remains the ſame, the increaſe in magnitude of the emerging pencil of rays, muſt depend upon an increaſe in magnitude of the pencil in the focus of the object glaſs.

—whence it is inferred that the moon's picture will be equally changed in ſize.

Conſequently, when the moon is viewed, if the aperture of the object glaſs be increaſed, the picture of the moon within the teleſcope, will be increaſed in the ſame ratio; but without increaſing the magnifying power of the inſtrument.

For the magnifying power is = the diameter of the aperture of the object glaſs divided by the diameter of the emerging pencil.

Demonſtration.

Let A = the diameter of the aperture of the object glaſs, B = the diameter of the emerging pencil, C = the focal diſ-

* Elementary Parts of Smith's Optics, page 93.

tance

tance of the object glafs, and D the focal diftance of the eye-glafs. Then $\frac{C}{D}$ = the magnifying power.

But C : D : : A : B. (Smith's Optics, p. 93.) And by divifion $\frac{C}{D} = \frac{A}{B}$. Therefore the enlargement of the picture of an object within the telefcope, does not increafe its magnifying power.

Hence thefe properties of the telefcope prove to a demonftration, that the conclufion which I have drawn from my experiments is a law of vifion; and this law fhows us, that no concavity of the fky; no terreftrial perfpective, nor even *the painting of the waves under Black-Friar's Bridge*, can explain the phenomenon in queftion *. The caufe lies wholly within the eye. Conclufion.

It is a moft unpleafant tafk to enter into controverfy, but I cannot fee how I could avoid it in this inftance. It appears to me, Sir, that you inferted C. L.'s paper for me to anfwer, and I have complied; hoping, however, that this is the laft time of my having occafion to ufe my pen, againft an anonymous writer. Common juftice feems to demand the names of thofe who undertake the examination of fuch papers as are owned by their refpective authors. Refpecting controverfy.

I am, Dear Sir,

Your moft obedient fervant,

EZ. WALKER.

Lynn, Jan. 18, 1805.

XII.

Defcription of a fimple Inftrument for making correct Drawings from Nature. *By* T. C. B.

To Mr. NICHOLSON.

SIR, *January* 22, 1805.

THE defcription of two inftruments for facilitating landfcape drawing from nature given in the firft volume, page 281, of your Journal, has fuggefted to me the conftruction of another,

* Philofophical Journal, vol. IX. page 237.

Inftrument for making correct drawings from nature.

another, which feems to unite the accuracy of the firft of thofe inftruments with the fimplicity and portability of the fecond. As I conceive it may be of fome fervice to thofe who are in the habit of fketching landfcape, I fend you a defcription of one which I have had made, that you may, if you think proper, publifh it in your Journal. Let A B, *Fig.* II. *Plate* 5. be a flat rule, fuppofe twelve inches long, having at its extremities two arms B D and A F turning upon a joint at B and A; and in each arm a circular joint at C and G: let the length of each arm B D and A F be ten inches: a handle to fix on E, and a thread paffing through two holes equidiftant from the handle, making any length, according to the angle under which the view may be beft feen.

To ufe the inftrument, take the end of the thread in the mouth, and hold the inftrument upright before the eye, then move either or both of the arms till the points D and F are brought in a line between the eye and any point in the landfcape you may wifh to delineate; lay the inftrument upon the drawing paper, and you will have the true fituation of fuch part of the fubject.—Proceed in like manner, taking care always to keep the bafe line in the fame place, till you get as many points as you require, by which means any landfcape or building may be drawn very expeditioufly, and with a great degree of accuracy.

To make the inftrument as portable as poffible, there is a joint in A B, which the handle covers, and the pieces D G and C F are made of thin brafs, to fold into the pieces A C and B G; fo that the inftrument, when folded up, need occupy no more room in the pocket than a fmall fpectacle cafe.

It is perhaps unneceffary to add that this inftrument may be ufed for the purpofe of copying, and anfwers the purpofe of a triangular or quadrangular compafs.

I am, Sir,

Your moft obedient and humble fervant,

T. C. B.

If the rough inclofed fketch of the inftrument is not fufficiently intelligible, Mr. Nicholfon may fee one at Mr. Frazer's, Optician, in Bond-Street.

Obfervations

XIII.

Obſervations on Baſalt, and on the Tranſition from the vitreous to the ſtony Texture, which occurs in the gradual Refrigeration of melted Baſalt; with ſome geological Remarks. In a Letter from GREGORY WATT, *Eſq. to the Right Hon.* CHARLES GREVILLE, *V. P. R. S. From the Philoſophical Tranſactions for* 1804, *p.* 279.

SIR, *Soho, April* 1804.

THE important geological conſequences that ſeem deducible from the changes of texture developed by Sir James Hall's very judicious experiments on the regulated cooling of melted baſalt, induced me to attempt a repetition of them, ſome time after the publication of his intereſting and ingenious paper. * I believe that formerly I had the honour of ſhowing you ſome of the reſults of my imperfect and diminutive experiments, which only ſerved to afford additional proofs of the tranſition from the vitreous to the ſtony texture, which takes place in the gradual refrigeration of glaſs. Circumſtances have prevented my reſuming theſe inveſtigations, till it lately occurred to me that ſomething might be learned, by expoſing to the action of heat, a much larger maſs of baſaltic matter than, as far as I am informed, had ever at one time been ſubjected to experiment.

Sir James Hall's experiments on the flow congelation of fuſed baſalt.

One of the common reverberatory furnaces uſed in iron founderies for the fuſion of pig-iron, was ſtrongly heated by a fire maintained for ſeveral hours. About ſeven hundred weight of amorphous baſalt, here called Rowley Rag, was broken into ſmall pieces, and depoſited gradually on the elevated part of the interior of the furnace, between the fire and the chimney, from whence, as it melted, it flowed into the deeper part, in which, in ordinary operations, the melted iron is collected. It was obſerved by the perſons attending, that it did not require half the quantity of fuel to fuſe the baſalt, that would have been neceſſary to melt an equal weight of pig-iron. When the whole was melted, it formed a liquid glaſs, rather

The experiment repeated on 7 cwt. of rowley rag.

It was eaſily fuſed, and formed perfect glaſs when quickly cooled.

• Publiſhed in the Tranſactions of the Royal Society of Edinburgh, Vol. V. and in our Journal, Vol. V. quarto ſeries.

 tenacious,

tenacious, from which a large ladle-full was taken, which, on being allowed to cool, retained the characters of perfect glafs. The fire was maintained, though with gradual diminution, for more than fix hours; after which time, the draught of the chimney was intercepted, the furface of the glafs was covered with heated fand, and the furnace was filled with coals, which were confumed very flowly. It was eight days before the mafs in the furnace was fufficiently cool to be extracted, and even then it retained confiderable internal heat.

Slow congelation.

Appearance of the mafs.

The form of the mafs, being given by the bottom of the furnace, was confiderably irregular, approaching to the fhape of a wedge whofe lower angles were rounded. It was nearly three feet and a half long, two feet and a half wide, about four inches thick at one end, and above eighteen inches at the other. From this diverfity of thicknefs, and from the unequal action of the heat of the furnace, too great an irregularity had prevailed in the refrigeration of the glafs, to permit its attainment of a homogeneous texture. Thefe circumftances might probably have been counteracted by better devifed precautions; but the inequality of the product is not to be regretted, fince it has fortuitoufly difclofed fome very fingular peculiarities, in the arrangement of bodies paffing from a vitreous to a ftony ftate, which might have remained unobferved, if the defired homogeneity of the refult had been obtained. I fhall now endeavour to defcribe the various products of this operation; and I fhall alfo fubmit to your confideration, fome remarks which appear to me to arife naturally from the phenomena I have obferved; premifing that, except where my opinions are fupported by the unequivocal demonftration of facts, I offer them with the utmoft deference to the decifion of more experienced and judicious mineralogifts and geologifts.

External characters, &c. of rowley rag.

It may be proper to give a concife defcription of rowley rag itfelf before I confider the products which it yields by igneous fufion. This fpecies of bafalt is fine-grained, of a confufed cryftallized texture; its fracture uneven in fmall pieces, conchoidal in large pieces. Its hardnefs fuperior to common glafs but inferior to feldfpar. Its tenacity confiderable. Its action on the magnetic needle ftrong, but without figns of polarity. Its fpecific gravity, according to my trials, 2.868. Its general colour iron gray, approaching to black. It is opaque; and it

reflects light from a number of brilliant points, some of which seems to be feldspar, and the others hornblende.*

1st. This substance is easily fused into glass, whose texture is completely vitreous, with few air-bubbles. Its fracture undulated conchoidal. Its hardness superior to feldspar, but inferior to quartz. It possesses scarcely any action on the magnetic needle. Its colour is black: it is nearly opaque, being translucent only in very thin fragments. Its specific gravity appears to be 2.749. Its glass.

2d. The tendency towards arrangement in the particles of the fluid glass, is first developed by the formation of minute globules, which are generally nearly spherical, but sometimes elongated, and which are thickly disseminated through the mass. The colour of these globules is considerably lighter than that of the glass; they are commonly grayish-brown, sometimes inclining to chocolate brown, and, when they have been formed near the interior surface of the cavities in the glass, they project, and resemble a cluster of small seeds. Their diameter rarely exceeds a line, and seldom attains that size, as, in general, they are so near to one another, that their surfaces touch before they can acquire considerable magnitude. In the process of cooling, they adapt their form to their confined situation, fill up every interstice, and finally present a First appearance of arrangement. Small globules appear in the mass.

* "The ragstone has been accurately analysed by Dr. Withering, who found that 1000 parts of it contained 475 parts of siliceous earth, 325 argillaceous earth, and 200 calx of iron; but this iron seems to me to be in a very small degree of calcination, from the dark blue colour of the stone, from the rusty colour it assumes on being exposed to a farther state of calcination by air and water, and from the magnetic property of the mountains, which, as Dr. Plot observed, turned the needle 6° from its proper direction. This magnetic property has since been observed in several basaltic mountains, particularly in the Giant's Causeway in Ireland, and very remarkably in a basaltic columnar mountain called Compass Hill, in the island Cannay, one of the Hebrides, described by George Dempster, Esq. in the Transactions of the Society of Antiquaries in Scotland, Vol. I." See Mineralogy of the South-west part of Staffordshire, by James Keir, Esq. F. R. S. published in Shaw's History of Staffordshire, Vol. I. Analysis of rowley rag by Dr. Withering.

Mr. Kirwan states the specific gravity of rowley rag, which he calls *ferrilite*, at 2.748; and assigns its melting point at 98° of Wedgwood's pyrometer.

 homogeneous

homogeneous body, wholly unlike glaſs, and equally unlike the parent baſalt. When the union of the little globules has been imperfectly effected, the fracture of the maſs indicates its ſtructure, by numerous minute conchoidal fractures, which diſplay the form of each globule. But, if the arrangement has extended a little farther, all theſe ſubdiviſions are entirely loſt; the maſs becomes perfectly compact, has an even or a flat conchoidal fracture, is nearly of the ſame hardneſs as the glaſs, is commonly of a chocolate colour, graduating into a browniſh-black, and the intenſity of the colour increaſes in proportion to the degree to which the arrangement has extended. Its aſpect is rather greaſy; and it much reſembles ſome varieties of jaſper in the compactneſs of its texture, and in its opacity. Its magnetic action is extremely feeble. Its ſpecific gravity appears to be 2.938.

It reſembles jaſper, and has little magnetic action.

More advanced appearance of the ſtony texture.

3d. If the maſs were now rapidly cooled, it is obvious that the reſult would be the ſubſtance I have juſt deſcribed; but, if the temperature adapted to the farther arrangement of its particles be continued, another change is immediately commenced, by the progreſs of which it acquires a more ſtony texture, much greater tenacity, and its colour deepens as theſe changes advance, till it becomes abſolutely black. Sometimes this alteration is effected by a gradual tranſition, the limits of which cannot be aſſigned, but more generally by the formation of ſecondary ſpheroids, in the heart of the compact jaſpedeous ſubſtance. Theſe ſperoids differ eſſentially from thoſe firſt deſcribed; the centres of their formation are more remote from each other, and their magnitude is proportionably greater, ſometimes extending to a diameter of two inches, and ſeeming only to be limited by contact with the peripheries of other ſperoids. They are radiated, with diſtinct fibres; ſometimes the fibres reſemble thoſe of brown hæmatites, and ſometimes they are faſciculated irregularly, ſo as to be very ſimilar in appearance to the argillaceous iron ores rendered priſmatic by torrefaction. They are generally well defined, and eaſily ſeparable from the maſs they are engaged in; and often the fibres divide at equal diſtances from the centre, ſo as to detach portions of the ſperoid in concentric coats. The tranſverſe fracture of the fibres is compact and fine grained; the colour black; and the hardneſs ſomewhat inferior to that of the baſaltic glaſs. When two

Darker colour.

Large ſpheroids of radiated texture.

two of the ſpheroids come into contact by mutual enlargement, no intermixture of their fibres ſeem to take place; they appear equally impenetrable, and, as neither can penetrate, both are compreſſed, and their limits are defined by a plane, at which a ſeparation readily takes place, and each of the ſides is inveſted with a ruſty colour. When ſeveral ſpheroids come in contact on the ſame level, they are formed by mutual preſſure into pretty regular priſms, whoſe diviſion is perfectly defined; and when, a ſpheroid is ſurrounded on all ſides by others, it is compreſſed into an irregular polyhedron.

Contact with ſpheroids produces polyhedrons.

4th. The tranſition from this fibrous ſtate to a different arrangement, ſeems to be very rapid; for the centre of moſt of the ſperoids becomes compact, before they attain the diameter of half an inch. As the fibrous ſtructure propagates itſelf by radiating into the unarranged maſs, the compact nucleus which ſupplies its place gradually extends, till it finally attains the limits of the ſpheroids; and the ſame arrangement pervades the matter comprehended between them. The maſs has now aſſumed a compact ſtony texture, and poſſeſſes great tenacity. Its hardneſs is ſomewhat inferior to that of the glaſs from which it was formed. Its action on the magnetic needle is very conſiderable. Its ſpecific gravity is 2.938. Its colour is black, inclining to ſteel gray: it is abſolutely opaque, and only reflects light from a few minute points. Though the diviſions between the ſpheroids are rendered imperceptible to the eye, they are not obliterated, and their ruſty ſurfaces are often diſcloſed by an attempt to fracture the maſs.

Third arrangement. Compact ſtony texture, great tenacity, opake, conſiderable magnetic action.

5th. A continuation of the temperature favourable to arrangement, ſpeedily induces another change. The texture of the maſs becomes more granular, its colour rather more gray, and the brilliant points larger and more numerous: nor is it long before theſe brilliant molecules arrange themſelves into regular forms; and, finally, the whole maſs becomes pervaded by thin cryſtalline laminæ, which interſect it in every direction, and form projecting cryſtals in the cavities. The hardneſs of the baſis ſeems to continue nearly the ſame; but the aggregate action of the baſis, and of the imbedded cryſtals, on the magnetic needle, is prodigiouſly increaſed. It appears to poſſeſs ſome polarity; and minute fragments are ſuſpended by a magnet. Its ſpecific gravity is ſomewhat increaſed, as it is now 2.949. The cryſtals contained in it, when

Granular texture.

Cryſtalline laminæ.

Polarity.

Increaſed denſity.

when examined by a microfcope, appear to be fafciculi of flender prifms, nearly rectangular, terminated by planes perpendicular to the axis; they are extremely brilliant; their colour is greenifh-black; they are harder than glafs, and fufible at the blow-pipe; they are fufpended by the action of a magnet. They are arranged nearly fide by fide, but not accumulated in thicknefs, fo that they prefent the appearance of broad thin laminæ; they crofs one another at all angles, but always on nearly the fame plane; and the lamina thus formed is often three or four lines long, and from a line to a line and a half broad, but extremely thin.*

It feems obvious, that an equalized temperature would have rendered the whole fimilar to the fubftance laft defcribed; and it may be fairly inferred, that by a continuance of heat, the minute cryftals would have been augmented in their dimenfions, by the acceffion of molecules ftill engaged in the bafis, or by the union of feveral cryftals, till they acquired fufficient magnitude for their nature to be abfolutely determined by the ufual modes of inveftigation. It is probable, however, if fuch precautions had been taken as might have fecured this degree of perfection in the ulterior refult, that the mafs would only have exhibited an uniform afpect, and that the interefting initial phenomena would not have been difcovered.†

There

* It may be obferved, that the cavities which exifted in the glafs are not obliterated during the fubfequent proceffes, though their interior furfaces undergo fome change. The minute globules firft formed often become prominent, and project into the cavities. Thefe minute points are foon obliterated by the large curves of the fibrous fpheroids, which give a mamellated form to the interiors of the cavities; and, when the cryftals are generated in the mafs, they fhoot into fome of the cavities, and line them with their brilliant laminæ.

† In this and the fucceeding paragraphs, the word molecule is ufed in the fenfe affigned to it by Hauy and Dolomieu, and is underftood to reprefent the peculiar folids, of definite compofition and invariable form, the accumulation of which, forms the cryftals of mineral fubftances. Such molecules, preferving their form and their effential characteriftics, may be extracted from moft cryftals by mechanical divifion, and may be fubdivided as far as our fenfes can recognife them. Though we cannot by mechanical means directly divide them into their elementary particles, we are enabled

to

There are ſome conſiderations which appear to offer a partial explanation of the formation of the globules, and of the radiated ſpheroids. It is well aſcertained that heat is emitted by all bodies, in their change from a gaſeous to a fluid ſtate, and alſo in their change from a fluid to a ſolid ſtate. It is reaſonable to ſuppoſe, that heat may alſo be emitted in thoſe changes of arrangement which affect the internal texture of a body, after it has attained an apparently ſolid ſtate. That a ſucceſſion of ſuch changes does actually take place, appears to me demonſtrated by the appearances I have deſcribed, and by the increaſe of ſpecific gravity, which ſeems to keep pace with the internal changes of the ſubſtance. It would appear, that theſe changes are cauſed by a gradual diminution of temperature, which permits certain laws to induce peculiar arrangements among the particles of the glaſs. When ſeveral of theſe particles enter into this new bond of aſſociation, they muſt form a minute point, from which heat muſt iſſue in every direction. That heat will gradually propagate itſelf, till the temperature of the glaſs is equalized; and then the recurrence of the circumſtances which induced the firſt particles to arrange, will cauſe other particles to arrange alſo, which the attraction of aggregation will diſpoſe round the point firſt formed. A ſecond emiſſion of heat in every direction will take place; the temperature will again be equalized; and again another concentric coat of arranged particles will apply itſelf to the little globule. But, at the time when the central point of this globule was formed, the equality of temperature, in the maſs of glaſs, would probably cauſe a number of ſimilar points to be generated. The formation of each muſt proceed in a ſimilar manner to what I have deſcribed, till their ſurfaces touch, and all the glaſs be converted into the ſame ſubſtance.

Explanation of the formation of the globules and ſpheroids; from the emiſſion of heat during congelation.

Theſe globules are therefore formed of concentric coats, but they are alſo radiated. Every one muſt have remarked

General connection between the radiated ſtructure and formation by concentric coats.

to effect this by chemical ſolution, the only power to which their aggregation yields. It will be evident, from the obſervations that follow, that I am inclined to adopt the ingenious idea of Dolomieu, that many apparently homogeneous rocks are compounds of the minute molecules of ſeveral ſpecies of minerals; and that, where a ſuitable opportunity is given, theſe will develope themſelves by the formation of their peculiar cryſtals.

the

the connexion that almoſt uniformly exiſts, between the radiated ſtructure and the formation by concentric coats. There are few radiated ſubſtances which are not diviſible into concentric fragments; and as few concentric arrangements which are not radiated. Of the firſt, it may be ſufficient to mention hæmatites; of the ſecond, calcareous ſtalactites. The tendency to this union of ſtructure, may perhaps be produced by the radiation of the emitted heat, or moiſture, if the ſolution be aqueous; and the diviſions of the coats will naturally take place at thoſe pauſes in the accumulation of particles, which the momentary emiſſion of heat neceſſarily induced.

Formation of the larger ſpheroids explained.

If this be allowed to explain the formation of the firſt ſeries of globules which conſolidate into the jaſpideous ſubſtance, it will alſo explain the formation of the larger and more diſtinctly radiated ſpheroids, which have been already ſtated to be very eaſily diviſible into concentric fragments. They probably were alſo formed round a central point, by the accumulation of thin coats; and the tendency to radiation, which ſeems almoſt inſeparable from this ſtructure, was perhaps aided by the arrangement induced by the emiſſion of heat from every part of the ſurface of the ſpheroids. This mode of formation has the advantage of explaining their impenetrability. Had they been generated by radii diverging from a centre, their compactneſs muſt have diminiſhed as their diameter increaſed; but, in the ſtructure which I have ſuppoſed, each coat is compoſed of particles ſolidly arranged in immediate contact with each other, leaving no ſpaces for penetration. The ſame progreſs is rigidly obſerved in the extenſion of the compact nucleus, which always occupies the centre of the radiated ſpheroids, and finally extends to their peripheries. It obſerves the concentric diviſions of the radiated part with the greateſt preciſion; and the line of their ſeparation is always perfectly defined. But the ſtate of aggregation into which the ſubſtance has now entered, is ſo perfect as to overcome the operation of the cauſes which formerly induced the fibrous ſtructure, and the maſs remains compact. The only change that the ſubſtance afterwards undergoes, conſiſts in the gradual accumulation of the cryſtalline molecules, and their arrangement, by their individual polarity, into regular ſolids. This depends on very different laws from thoſe

thofe which confolidated the fluid glafs, and aggregated its particles into a compact uniform ftone.*

Other confiderations. Remarkable circumftance, that thefe changes fucceed after fluidity may be fuppofed to have ceafed, &c.

The appearances that I have endeavoured to defcribe, feem deferving of confideration in feveral points of view. Few things can be more at variance with commonly received opinion, than the diverfified fucceffion of changes of ftructure which this glafs exhibits in its paffage to a cryftallized ftate. The generation of the globules which unite to form the jafpideous fubftance, is what we might be prepared to expect, by obferving the cooling of a common iron furnace flag. But it appears not very obvious to common apprehenfion, that the fpecies of arrangement requifite to form this intermediary fubftance, could be compatible with any fluidity permitting farther motion of the molecules of the mafs; yet, immediately after the completion of this arrangement, they receive a new difpofition, and the radiated fibrous ftructure commences. Sometimes this pervades even the unaltered glafs; but I prefume this only to happen where the minute globules firft formed were fcattered fo far afunder, that their centres became fibrous, before their peripheries came into contact. This view of the fubject is juftified by the analogous operation of

* The cafe is confiderably different, where cryftals poffeffing regular forms are generated in glafs. The molecules of which they are formed, have doubtlefs been only fufpended in the vitreous medium; and their union is determined by cryftalline polarity, which appears to me perfectly diftinct from the fimple aggregation which changes a fluid into a folid, whether it be homogeneous or compound, which affects the internal arrangement of thofe bodies, but which never can feparate their components into diftinct maffes, or form them into regular folids. Every molecule, at the moment of its formation, muft neceffarily be endowed with all the properties it afterwards poffeffes. The fufpenfion of fuch molecules in a fluid medium, though it may conceal, cannot alter thofe properties; and the union of fuch molecules, to form a regular folid, in no refpect alters their individual or aggregate qualities. Whether heat be evolved at the moment of this union, is a queftion not eafily folved; as the cryftallizations with which we are familiar are from chemical folutions, in which fome of the molecules are generated by the feparation of a combined fubftance, at the moment when others are united by cryftalline polarity.

the

the formation of cryſtals, ſimilar to thoſe deſcribed, in the heart of the radiated ſperoids, while their exteriors ſtill retained the fibrous texture.

— Review of the facts under this point of view.

If it be conſidered as extraordinary, that a change ſhould be effected, converting an apparently ſolid and homogeneous maſs into an accumulation of radiated ſpheroids, and that theſe radii ſhould loſe their fibrous ſtructure, and aſſume the texture, aſpect, and tenacity, of a compact, hard, and homogeneous ſtone, it is certainly much more extraordinary, that this ſtone ſhould permit farther arrangement to proceed, and ſhould enable the cryſtalline molecules which it contains in a ſtate of confuſed aggregation, to arrange themſelves, and to form cryſtals which, although minute, are equal in the perfection of their forms, and in the brilliancy of their natural poliſh, to the moſt precious products of cryſtallization. It is alſo well deſerving of obſervation, by how regular a march the magnetic influence of the ſubſtance keeps pace with the perfection of its arrangement, till it becomes ſo powerful, that fragments of the regenerated ſtone are ſuſpended by the attraction of a magnet.

The act of cryſtallization is incompatible with ſolution or fluid combination.

It has been moſt juſtly remarked by Mr. Smithſon, that ſolution, far from being neceſſary to cryſtallization, effectually prevents its commencement; for, while ſolution ſubſiſts, cryſtallization cannot take place. It may remain a queſtion, whether previous ſolution be eſſential, as a preparatory means of obtaining, by ſubſequent evaporation, or cooling, the ſmall parts of bodies diſengaged, ſo that they may unite to form regular cryſtals. If by ſolution be only meant, that ſimple action of heat, or water, which merely counteracts the force of aggregation, and relieves the molecules from their bonds of union with each other, it certainly is a requiſite; but if by ſolution be meant, that action of affinities by which not only the force of aggregation is overcome, but the combinations which conſtitute the molecules are deſtroyed, it obviouſly is not only unneceſſary but prejudicial to cryſtallization; as a new ſet of molecules muſt be formed, by a new combination of the elementary particles, before the formation of regular bodies can commence.

Suſpenſion is neceſſary; but this may be called mechanical.

The ſuſpenſion of the molecules ready to cryſtallize, may be correctly ſaid to be merely mechanical. Though the mechanical action of trituration can never be expected to reſolve even the moſt eaſily diviſible body in its molecules, becauſe the

the fractures will be at leaft as frequently acrofs the natural joints as in their direction, yet, even by this rude method, fome perfect molecules may be diffengaged; for we find, that water paffing over large furfaces of filiceous fand, finds fome molecules of filex in the ftate proper for aggregation, and even for cryftallization. Mechanical fufpenfion in a fluid medium, of fuch denfity that the cryftalline polarity may be enabled to counteract the power of gravity, is with juftice confidered by Mr. Smithfon the only requifite for the formation of cryftals. * The circumftances I have detailed, appear to me an additional confirmation of this remark, and perhaps go ftill farther, by fhowing that even the fluidity (in the common fenfe of the word) of the fufpending medium is not an indifpenfable condition. For it appears impoffible to annex the idea of fluidity to the union of the minute globules which form the jafpideous fubftance, ftill lefs to that fubftance when formed, and ftill lefs to thofe fpheroids whofe obftinate impenetrability is fo ftrongly defined. And if, by any power of imagination, thefe can be fuppofed to be fluid at the time they retain this conformation, how can it be fuppofed that the compact hard tenacious ftone into which they are changed could retain thefe characters in a fluid ftate? Yet the fubfequent formation of cryftals proves, that either all thefe contradictions muft be, or that the particles of bodies apparently folid muft be capable of fome internal motion, enabling them to arrange themfelves according to polarity, while they are folid and fixed, as far as they have reference to the ordinary characters of fluidity.

Even fluidity does not feem an indifpenfable condition.

Inftances even more remarkable have very long been known and authenticated, though perhaps they have not been generally regarded with the attention then deferve. Glafs veffels are well known to be convertible into Reaumur's porcelain, by the internal arrangement of their particles, without lofing their external form, and confequently at a temperature very much below that requifite for their fufion, The change of glafs into Reaumur's porcelain, does not arife from an evaporation of the alkali, as has been alledged, but from a regular arrange-

Inftances of the internal motions and arrangements of particles of bodies at temperatures below fufion.

Reaumur's porcelain fully defcribed:

* See a chemical Analyfis of fome Calamines, by James Smithfon, Efq. Philofophical Tranfactions for 1803, page 27. See alfo Dolomieu, *Journal des Mines*, No. 22, page 53.

ment

ment of the molecules of the glafs. It commences by the formation of fibres perpendicular to the furface of the glafs, and penetrating into it. At nearly the fame time, fmall radiated globules are formed in the interior of the glafs, and the union of thefe with the fibres, by their mutual increafe, forms the whole into a new fubftance; and, if the requifite temperature be longer maintained, the fibres difappear, and the whole becomes fine-grained, and almoft compact. This fubftance, from the improved ftate of its aggregation, is much ftronger and more tenacious than before, and is not fufible at a heat fufficient to fufe the glafs it was formed from; but, if that aggregation be once deftroyed, the glafs refulting from its fufion is equally fufible with the original glafs; and a repetition of the procefs will again form Reaumur's porcelain, which may be again fufed, and fo on repeatedly, for the quantity of alkali evaporated during the operation is extremely fmall. The hardnefs and brittlenefs of metals rapidly cooled, contrafted with the foftnefs and tenacity refulting from their gradual refrigeration, are all analagous inftances; and all the proceffes in which annealing is employed, and more remarkably the tempering of fteel, the proofs of the internal motions and arrangements of the particles of matter, at temperatures very much below the heat is requifite for their fluidity.

hardening; tempering; annealing:

Cryftallization of a mafs of the fame nature throughout;

Whatever doubts may arife refpecting the formation of the cryftals, there feems no reafon to fuppofe that their gradual increafe would ceafe, till all the molecules belonging to that fpecies were exhaufted, if the temperature favourable to their generation was continued. If the mafs was entirely compofed of one fpecies of molecules, it would be refolved into an aggregation of cryftals of the fame fubftance; and probably by a ftill farther continuation of the procefs of arrangement, into one cryftal, which, though it might not poffefs a regular external form, would be perfect in its internal ftructure.

of particles differing from each other.

But, if the mafs contains two diftinct fpecies of molecules, different refults muft take place, which will be modified by the proportional quantities of the components. As it has been demonftrated by Berthollet, that the attraction of maffes of matter are relatively as their quantities, it follows, that unlefs a very potent counteracting caufe be exerted, the moft abundant ingredient in the mixture will be the firft to cryftallize.

Generally, the moft abundant ingredient will cryftallize firft.

2 But

But this cryſtallization will not comprehend the whole of its molecules; for, after a certain quantity of them are arranged, the proportions of the remaining fluid are altered; that ingredient which was before the leaſt, may now be equal, or even greateſt, and it will exerciſe its attraction. As the firſt cryſtallization, by ſubtracting a large portion of the fluid particles, muſt have obliged the molecules of the leſs abundant ſubſtance to approach each other very cloſely, they may be able to collect themſelves entirely in their firſt attempt to cryſtallize, or they may form alternate cryſtallizations with the remaining unarranged molecules of the more abundant ſubſtance. However various the ſpecies of molecules may be, they will be regulated by analogous aws, and only ſerve to diverſify the generated ſubſtances.

Diverſities.

The moſt infuſible cryſtals would not neceſſarily be formed firſt.

It by no means follows, that the cryſtals afterwards found to be moſt infuſible would be firſt generated. Their formation does not altogether depend on their greater or leſs fuſibility, but on the relative ſtrength of the attraction which unites them to the matter they are immerſed in, and of the polarity which invites them to cryſtallize. In all cryſtallization from compound fluids, the order in which the ſeveral bodies cryſtallize muſt be determined by their relative quantities and attractions. It is perfectly obvious, that no molecules can form a cryſtal in a heat ſufficient for its fuſion; but it by no means enſues, that it will be formed as ſoon as the molecules are cooled to the point where the cryſtalline polarity overcomes the diſintegrating power of heat; for they may remain ſuſpended in a fluid formed by more fuſible bodies, provided this fluid be ſufficiently abundant to keep them from contact with each other, for the cryſtalline polarity appears to exert itſelf only at extremely ſmall diſtances. In a maſs compoſed of ſubſtances in a ſtate of fluidity, with refractory molecules ſuſpended among them, it is pretty clear, from the preceding paragraph, that the moſt abundant ingredient will be the firſt to cryſtallize. But the removal of a portion of the ſuſpending fluid muſt bring the refractory molecules nearer together, and perhaps ſo near that the cryſtalline polarity may overcome the attraction of the fluid for them; they will therefore cryſtallize next, and will be followed by the remaining ingredients, in the order their attractions dictate.

As

When refractory fubftances cryftallize after more fufible matters, they may have the impreffion of thefe laft.

As the cryftals laft formed muft neceffarily be impreffed, at the parts in contact, by the peculiar forms of thofe which have been firft generated, it alfo follows, if the preceding reafoning be juft, that the infufible cryftals may be found impreffed by the more fufible fubftance, which cryftallized firft; and the remaining ingredients of the mixture, which were fubfequently arranged, may be moulded on the refractory cryftals; and thus, in the fame fpecimen, may exift a refractory fubftance generated by fire, impreffed by more fufible bodies, and impreffing them in its turn. From the fame confideration it is obvious, that no cryftal can be formed at a temperature above the degree of its fufibility; and that, as a neceffary confequence, no cryftal which is more fufible than the bafis in which it is imbedded, can be formed by igneous operation.

No cryftal more fufible than its enveloping mafs, can be formed by igneous operation.

Aqueous folution and fufpenfion do not differ from thofe in the dry way. They are effected by the fufion of the folid water, or ice, &c.

The fame laws muft regulate the arrangement of aqueous folutions, and of molecules fufpended in aqueous folutions. All thefe are dependant on heat; for we are unacquainted with any fluidity, and confequently with any folution, which heat does not produce. Ice and foda have no more action on each other than foda and quartz: raife the temperature of the ice, and it unites with the foda; raife the temperature of the foda, and it unites with the quartz. Both folutions are effected by heat, of the degrees of which we know neither the beginning nor the end, and are therefore utterly unable to eftimate what aliquot part of its fcale is adequate to the production of thefe effects. Probably a very minute one.

(To be continued.)

XIV.

Account of the Method of bleaching Cotton, as practifed at Salzburgh; and the Art of giving a permanent Red to Cotton and Linen. *By* M. C. Schoerbing.*

Salzburg method of bleaching cotton thread, &c.

COTTON thread is always wafhed before it goes to the weaver. The method of wafhing here to be defcribed gives it a much whiter colour than ordinary; and it is equally appli-

* Journal of Van Mons, No. 15.

cable

cable to cottons in the piece. This method is called, for what reafon I know not, the Salzburgh method, though it is chiefly practifed at Reginfburgh, where four families have long kept it a fecret, which has not till the prefent occafion been divulged. This wafhing renders the cotton more flexible; the goods made from it are of a clofer and more even texture; the cotton does not require any fubfequent bleaching, and when it comes afterwards to be dyed, it takes a finer, more permanent and more uniform colour. The operation is performed with foap, water and ftrong leys, in the following manner:

Salzburgh method of bleaching cotton thread, &c.

The articles to be wafhed, which ufually confift of thread, ftockings, night caps, and handkerchiefs, are evenly difpofed in a large veffel or tub. The bottom of this veffel is firft covered with a coarfe linen cloth, upon which are laid firft the handkerchiefs, afterwards the ftockings and night-caps, and laftly the thread. The whole is then covered with a fecond coarfe cloth of a clofe texture, and very clean, which defends the mafs from the contact of the air. The veffel itfelf refembles thofe ufed for lixiviation in the foap work of Konfholm. A boiler having its diameter at the top $1\frac{1}{2}$ Swedifh ell (forty inches) and at the bottom $1\frac{1}{4}$ ell (34 inches) and its depth one ell (27 inches) is filled with water to which a fufficient quantity of cauftic leys of potafh is added to make the fluid produce a greafy feel between the fingers, and afterwards 2lbs. of fliced foap of Rigenfburg. This liquid is made to boil and poured upon the cotton; a fhort time after which it is drawn off, to be again heated, during which interval a hot folution of foap is continually poured upon the cotton. Thefe operations are repeated for four or fix hours, or until the cotton is well foaked, and very hot. It is then left to fteep for twenty-four hours in foap-water. This procefs is ufually performed between midnight and fix in the morning; and the next morning the fluid is drawn off, the cotton well wafhed and the water preffed out by a fcrew-prefs. This conftitutes the firft wafhing.

After the cotton has been well cleanfed in this manner, the principal wafhing is effected as follows:

The wafhed cotton is difpofed in the veffel as before, with this difference only, that the mafs is enveloped in cotton cloth inftead of linen, which is lefs capable of refifting the action of the cauftic leys. A clear and colourlefs ley is prepared with

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Salzburg method of bleaching cotton thread, &c.

two thirds wood aſhes and one third lime, and concentrated until an egg deſcends ſlowly in it (ſpecific gravity about 1.081). This ley in a boiling ſtate is poured upon the cotton; after which it is drawn off, again heated, and again poured on; and this repetition is continued from midnight to noon, or for twelve hours. The whole of the ley is then drawn off, and the cotton taken out of the veſſel while hot, with the hands defended by gloves, and after ſpreading it upon a table, it is again put into the veſſel, but in a reverſed order. The ſame waſhing is repeated with other leys and continued for twenty-four hours.

Laſtly, the boiler is filled with water, to which 2lbs. of ſoap are added, and the liquid brought to the boiling heat. The cotton is waſhed with this water, conſtantly kept boiling for twenty hours, and is afterwards left to ſteep for ten or eleven hours. The fluid is then drawn off, the cotton taken out of the veſſel, and ſpread on a long wooden table, where it is waſhed and beetled, and afterwards waſhed in a running water. Laſtly, the water is preſſed out by means of a preſs, and the cotton dried upon ſtaves in the ſun, or in damp weather in a room appropriated to that purpoſe.

The remaining leys after theſe waſhings cannot be uſed a ſecond time, but have a blood-red colour; they are mixed together, and are uſed in the common operations of the laundry. The ſoap waters are thrown away.

The ſtaves on which the cotton is ſuſpended muſt be waſhed every time. It is preferable, when the weather permits, to ſuſpend each piece by threads and ſtretched cords in the open air. The pieces ought not to be brought too near together, for fear leſt the places to which the air has not free acceſs ſhould be ſpotted with brown ſpots.

Piece goods and raw linen thread will receive a partial bleaching by this method. It is neceſſary however to make the leys of only half the ſtrength, and the lixiviation muſt be continued only half the time, for fear of weakening the texture.

Linen and cotton as vegetable ſubſtances have the defect of not taking fixed colours. The cauſe reſides in their reſinous principle, of which they muſt be deprived. Alcohol would be a ſure and eaſy ſolvent for this principle, but it is too expenſive. After alcohol, the beſt ſolvents of this principle are oils

oils of every defcription, but particularly fifh oil. The cotton is foaked in this oil for 24 hours, after having again boiled it for two hours; it is then run and fufpended in the air for fome days, in order that the oil may be well feparated, and it is entirely deprived of the oil by a ftrong hot ley with fubfequent wafhing and drying. For this effect a ley of two thirds afhes and one third lime may be ufed, with the fame quantity of fheeps dung as of both the before-mentioned ingredients. The cotton is not only boiled in this mixture, but it is alfo applied feveral fucceffive times; the goods being wafhed in clear water between each lixiviation. The cotton thread may alfo be wafhed in water containing a fufficient quantity of potafh to render it greafy to the feel. The thread is to be boiled for twelve hours in this ley, or till the fluid becomes black. Laftly, the thread is wafhed and fteeped in water acidulated with weakly fulphuric acid, it is to be taken out of this fluid after remaining in it at leaft an hour, and then wafhed and dried.

If this method of bleaching were once generally known, it would not be neceffary to purchafe the article of the bleacher, or to fend cotton to them to be bleached. In order to afcertain whether cotton bleached in any manner whatever is perfectly deprived of its refin, and proper to receive the dye, the glafs is to be filled with water, and a thread of the cotton placed on the edge of the glafs, fo that half its length fhall be within and half without. If the former end finks in the water and the whole thread acts as a fyphon, by caufing it to run over, it will be a proof that the cotton will have the requifite purity.

The cotton thus purified muft pafs through three mordants: Red dye for cottons.
1. A decoction of nut-galls. 2. A folution of tin; and 3. alum water.

For the decoction of nut-gall. For 1lb. of cotton or linen thread. For the former half a pound of the dark coloured nut-gall, or for the latter 1lb. is taken. The nut-gall is grofsly pounded and boiled with a handful of birch leaves in a copper boiler, with two and half pots of rain water till reduced to one half. The liquid being left to fettle and poured while yet hot through a cloth on the cotton, this is fuffered to fteep for 24 hours, frequently working or preffing, in order that it may be univerfally penetrated with the galling principle. It is then

 taken

Red dye for cottons.

taken out, rung, and dried by fufpending it in the open air, or in a heated chamber. It is needful only to take care that no water fhall drop upon the cotton, as that would produce fpots in the dye. In this operation the thread muft be clofely packed together, in order that the nut-gall may uniformly penetrate its mafs. It is then conveyed into a folution of tin, made as hereafter defcribed, and the decoction of nut-gall muft be kept to be ufed in the aluming. The folution of tin is prepared in the following manner: Sal ammoniac or fea falt in fine powder is to be diffolved to faturation in 1lb. of aqua-fortis; 2 oz. of fine Englifh tin rafped, are added to this, or as much of the metal as the acid can diffolve to faturation. In another veffel, 2 oz. of fea-falt are to be diffolved in one pint of rain water, and the folution of tin is to be poured drop by drop with continual agitation into this falt water. The galled thread is afterwards put into a ftone ware veffel, clofely preffed together, and the laft mentioned mixture poured thereon. The thread muft be occafionally compreffed with the hand, in order to affift the penetration, and it muft then be covered from the air and left to fleep for twenty-four hours, at the end of which time it is taken out, rung and dried in the air or in a heated room, where it may remain for 48 hours. After this it is wafhed in pure water again, dried, and emerfed in the alum-water, of which we fhall proceed to defcribe the compofition.

The remaining folution of tin is referved for a fubfequent operation, for which, in that cafe, no more than three quarters of a pound of aqua-fortis and the other ingredients in proportion, need be taken for 1lb. of thread.

The alum water is made as follows: Whatever may be the kind of alum, it muft be previoufly calcined. One pound of the crude alum is required for 1lb. of thread. When it is calcined it is pulverized and diffolved in one pint or Englifh quart of water, and rather more than an equal meafure of the remaining decoction of nut-gall is added. Thefe being well mixed and heated, are to be poured on the thread, which has been treated as before with the folution of tin. It is left for fourteen hours in this bath and afterwards preffed and dyed.

The dying 1lb. of good bruifed or ground madder previoufly foaked for fome hours in water, is put into a boiler of fufficient fize, which is to be filled with water and placed on

a moderate

a moderate fire. As ſoon as the contents begin to heat, the thread is put in and continually turned. It is requiſite that the thread ſhould have been previouſly ſteeped in water, and afterwards wrung, in order that it may be very equally penetrated with the colour. The thread is left in the bath till the liquor boils, after which it is taken out, well waſhed, the threads are arranged on the hand, and the water preſſed out. This firſt dyeing gives the thread a pale red colour. The boiler is then emptied and again filled with water, into which another pound of madder previouſly ſoaked in water is put, and this bath is heated. As ſoon as it has become hot, the thread is put in and boiled for one quarter of an hour. Care muſt be taken that the heat be not too ſtrong. The firſt bath ſhould ſcarcely boil, for fear the articles ſhould take a brown colour; but this inconvenience is not to be feared in the ſecond bath, when the thread is already penetrated with a red colour. After the thread is well waſhed for the ſecond time, it ought to have a deep brown colour; if it has not it muſt be again boiled with a quarter of a pound of madder. After the laſt dye, the thread is to be waſhed by hand in hot ſoap water, after having waſhed it in cold water, and it is then to be dried.

Red dye for cottons.

XV.

Report on an artificial Production of Camphor, announced by M. KIND. *Read to the Society of Pharmacy by* BOULLAY.*

BEING charged, in conjunction with M. M. *Cluzel* and *Chomet*, to give an account to the Society of an intereſting phenomenon announced by *M. Kind*, in *Trommſdorff's Journal de Pharmacie*, extracted and reported in the 14th number of the *Journal de Phyſique et de Chimie*, publiſhed by *Van Mons*, I now inform them of the reſult of our labours.

"For the purpoſe of procuring the arthritic liquor of *Pott*, which is preſcribed here with advantage, for reſolving the noſities and other obſtinate tumours which form upon the joints, I intended," ſays *M. Kind*, "to incorporate the muriatic gas

M. Kind's experiment of the production of camphor by common oil of turpentine to abſorb muriatic gas.

* From Annales de Chimie, Fructidor, No. 153. Vol. LI.

with

with oil of turpentine, at the inſtant of the diſengagement of the gas. This operation ſhewed me a phenomenon which, I am of opinion, is deſerving of being known.

" I put decrepitated muriate of ſoda into a tubulated retort; I adapted two of Woulf's bottles to the retort, into which I had put a quantity of oil of turpentine, equal in weight to the ſalt. On this I poured half the proportion of concentrated ſulphuric acid, and I diſengaged the muriatic gas by a graduated fire. At firſt the oil acquired a yellow colour, afterwards a pale brown colour, which, towards the end turned to a deep brown. After cooling, the liquor was almoſt wholly coagulated into a cryſtalline maſs, which, in every reſpect, comported itſelf like camphor.

" I do not think," ſays M. Kind, " that a ſimilar production of camphor has ever been noticed before; but Meyer ſpeaks of a concretion of a camphoric nature, formed in oil of turpentine digeſted with dry cauſtic ſalt." He afterwards inſtances a formation of camphor obſerved by the late M. Trommſdorff, on diſtilling eſſential oils on lime. " It is probable," ſays M. Kind, " that the great tendency of muriatic gas to combine with water, determined the union of the principles of this liquid contained in the oil, ſo that the carbon became predominant, and made the oil brown; and, that the effect of this acid gas on it, correſponded with that obſerved by Achard, in the action of concentrated ſulphuric acid on fat oils. He did not remark that the heat was as conſiderable during the action of the gas as Woulf ſtated it to be."

Repeated by M. Trommſdorff.

In an additional note, M. Trommſdorff announces that he had repeated M. Kind's experiment, and that he had obtained analogous reſults.

He dried the concrete matter between ſheets of paper; it became very white, ſemi-tranſparent, and poſſeſſed the following properties:

" Its odour was ſtrong, very analogous to that of camphor, but injured by that of the turpentine.

" Placed in a ſilver ſpoon, and heated over red hot coals, it volatilized without leaving any reſidue, and yielding a ſtrong ſmell of camphor.

" Its vapour inflamed.

" Expoſed to a gentle heat, in a ſmall phial, it was entirely ſublimed. It diſſolved with facility in oil of almonds.

It

"It was entirely diffolved by alcohol, but not fo flowly as common camphor, and was precipitated unaltered by water.

"Concentrated nitric acid affected its folution, at firft tranquilly, but afterwards with a difengagement of nitrous gas and heat. The mixture became turbid by the addition of water, but did not depofit camphor."

He concludes from thefe experiments that this product comported itfelf exactly like camphor, except with the nitric acid; but he believes that this difference may depend on a fmall quantity of muriatic acid, or a little oil of turpentine, from which it is very difficult to free it entirely.

M. Trommfdorff afterwards difengaged the gas from a quantity of muriate of foda equal to that employed in the firft experiment, through the matter remaining in the bottle; but he found that inftead of augmenting, the camphorized part diminifhed in quantity, loft its camphoric odour, and the alteration of its other properties induced this chemift to believe that the new addition of gas had caufed it to undergo a decompofition.

Repetition of thefe experiments by the reports.

The above experiments, repeated with the greateft attention, gave us fatisfactory refults, even with the different fpecies of oil of turpentine of commerce.

The importance of the fubject, and the objections made by fome members, at the reading of the obfervation which lead to this report, made us fenfible how much the labours of M. Kind left to be wifhed for; the quantities of the product alfo not being indicated, we thought we fhould accomplifh the intention of the Society, in following the track marked out by the author, if we endeavoured to add fome facts to thofe he has publifhed, and we propofed the following queftions for our guidance.

Queftions for inveftigation.

1ft. Does the gazeous muriatic acid ferve to determine the re-action of the principles of the oil of turpentine, and to change their proportion, to form the camphor; or, is its action limited to infulating this immediate principle, from the oil with which it was not combined?

2nd. What is the quantity of the camphoric product?

3rd. Will a greater quantity of the gas than that indicated by M. Kind add to the product, or will it deftroy it?

4th. Will another quantity of gas added to the liquor which has yielded the camphor, and from which it has been feparated, determine a new formation of this principle in it?

5th. What

5th. What are the beſt means of purifying this camphor, and of freeing it from the odour which it retains?

6th. What are the relations and differences between the purified camphor of turpentine, and that of the camphor-laurel?

7th. Would liquid muriatic acid produce the ſame effect?

8th. Would the other acids act in the ſame manner on this oily ſubſtance?

The complete ſolution of theſe different queſtions would doubtleſs have required a great number of experiments, to which we were unable to attend: we ſhall only relate thoſe which we made.

Apparatus.

With a view to obtain a more conſiderable product, which would yield us the concrete matter in a quantity ſufficient to be aſcertained and ſubjected to different experiments, we prepared an apparatus ſimilar to that uſed by the author of the diſcovery, but of larger dimenſions. It conſiſted of a tubulated retort, and two Woulf's bottles, provided with their tubes of communication and ſafety. Four pounds of marine ſalt, and two pounds of concentrated ſulphuric acid were introduced into the retort; four pounds of very white oil of turpentine were poured into the firſt bottle, and two pounds of diſtilled water into the ſecond. Fire was placed under the mixture, and augmented gradually till the entire diſengagement of the muriatic acid gas, which was totally abſorbed by the oil. The latter exhibited the following phenomena: it became at firſt of a citron colour, and afterwards browniſh, which grew gradually darker towards the end of the operation; it retained its tranſparency; it grew very hot; its volume augmented about a tenth; the ſides of the bottle were covered with ſmall limpid drops which united ſlowly at its bottom, in the form of a thick oil.

Proportion of ingredients.

All the gas was abſorbed. Changes of the oil.

Cryſtalline product by repoſe.

Left to itſelf for twenty-four hours, it formed a maſs of irregular cryſtals, from which ran a brown liquid. When dried and ſtrongly preſſed between ſheets of paper, this cryſtalline matter was very white, and of a peculiar odour, participating of the oil and the acid made uſe of; it weighed twenty-four ounces.

More cryſtals by cold.

The liquor which had been ſeparated from theſe cryſtals, expoſed in a cellar for ſeveral days, yielded others, which dried like the firſt, were of equal whiteneſs, and weighed four ounces.

To

To afcertain whether there did not yet exift a further quantity of this concrete matter ready formed, in this fort of mother-water of camphor, it was expofed to an artificial cold of eight or ten degrees below 0, during feveral hours. By this means it yielded other cryftals, which, collected like the preceding, offered the fame characters, and weighed two ounces; this brings the quantity of camphor to feven ounces and a half for each pound of oil of turpentine.

Seven and half oz. of camphor from one pound of oil of turpentine.

This liquor, not appearing capable of yielding more cryftals, was divided into two portions, one of which was referved for examination in this ftate, the other was fubjected to the action of a new quantity of acid gas; it diffolved a part of it, and became almoft black, without lofing its tranfparence, even after remaining for feveral days in the cellar.

More gas did not produce cryftals in the refidual liquor.

Another dofe of oil of turpentine was charged with a proportion of gas, double that mentioned above. The precipitation of camphor took place in nearly a fimilar quantity; the liquor was very little more coloured, but a much greater quantity of fat oil was formed.

Double quantity of gas.

An ounce of this matter wafhed in diftilled water, and put in a filter to drain, became of the moft beautiful white; it no longer gave figns of acidity, but had ftill an odour of turpentine. The water in which it had been wafhed was very aromatic; it reddened the tincture of turnfole, and precipitated by nitrate of filver.

Action of diftilled water on this camphor;

An equal quantity of the fame fubftance was wafhed with water impregnated with a fiftieth part of unfaturated carbonate of potafh: it loft much of its firft odour.

and of water acidulated with carbonate of potafh.

New portions of this rough camphor were mixed in equal parts, one with charcoal in powder, another with very dry afhes, a third with quick-lime, a fourth with alumine *(porcelain earth)*, and were introduced feparately into fmall glafs alembics, the capitals of which were, by a gentle heat, rapidly covered with diftinct groupes, compofed of a multitude of fmall needle-formed cryftals, diverging from the fame center.

Action of charcoal, afhes, quick-lime, and alumine.

Purified in this manner, it entirely loft the odour of oil of turpentine; that which it retained was more analogous to common camphor, but not fo ftrong. In this ftate it fwims on water, to which it communicates its tafte; it burns on its furface, without experiencing the revolving motion obferved by Profeffor

Other properties.

Profeſſor Venturi. The only motion we obſerved was the reſult of the molecular attraction, which determined a great number of the fragments diſperſed over the ſurface of the water to collect in one point.

Is ſoluble in alcohol.

It is wholly and readily diſſolved in alcohol, from which water ſeparates it unaltered.

Action of dilute nitric acid.

Nitric acid, at thirty degrees of *Beaumé's* areometer, poured on this new camphor, had no action on it, even after ſeveral days of contact; although the ſame acid diſſolves the common camphor with the greateſt facility, becoming covered with an oily ſtratum, and water precipitates pure camphor from it.

Of concentrated nitric acid.

Very concentrated nitric acid had not, at firſt, any action on our camphor; but after a few ſeconds the ſolution is effected, and at the ſame time a diſengagement of nitrous gas is manifeſted: water rendered this ſolution very ſlightly turbid. The ſame acid, on the contrary, diſſolved the camphor of the ſhops rapidly, diffuſing white vapours. This ſolution took a beautiful fire-red colour, and water ſeparated pure camphor from it in flocks.

It is not diſſolved by acetic acid.

Neither did acetic acid, which perfectly diſſolves the common camphor, effect the ſolution of this. By heat, it ſoftened and ſeemed to diſſolve; but on cooling, it was wholly collected on the ſurface of the liquid, with all its properties.

We afterwards examined the liquor called the *mother-water of camphor.*

Phyſical properties of the mother-water. Peculiar oil.

This brown tranſparent liquor, lighter than water, heavier than alcohol, fuming by contact of the air, of a peculiar odour, ſwam above a thick reddiſh oil, the ſmall quantity of which obliged us to neglect the examination of it.* The taſte of this mother-water was ſharp; it did not mix with water, nor yield its acid to it. Agitated in this liquid, its particles remained diſperſed for a long time, before they collected on its ſurface.

Soluble in alcohol and ether.

Alcohol and ether diſſolved it completely; but the addition of water ſeparated them.

Inflammable.

Inflamed in a capſule, it burns without a reſidue, diffuſing a conſiderable quantity of very odorours ſmoke.

Action of ſulphuric acid,

Concentrated ſulphuric acid poured into this liquor, blackened it, and diſengaged from it ſome vapours, and a very ſtrong odour of gazeous muriatic acid.

* We only aſcertained its ſolubility in alcohol and ſulphuric ether, and its inſolubility in water.

A ſolution

A solution of crystallized carbonate of soda produced a slight effervescence in it, discoloured it, rendered it lighter, and formed muriate of soda.

and of carbonate of soda.

This muriated oil, distilled to dryness in a suitable apparatus, yielded some muriatic gas, some drops of thick oil, similar to that mentioned above, a very light and very combustible perfumed liquid, which comported itself with water, alcohol, sulphuric acid, and soda, in the same manner as before distillation; there was only less muriatic acid in it. The residue was a black matter, glutinous like pitch, dissolving imperfectly in alcohol, and yielding muriatic acid to water.

Products by its distillation.

We afterwards made a mixture of equal parts of oil of turpentine and muriatic acid at 25°. We agitated it from time to time, and then left it for several days. The oil took a reddish brown colour, the acid also became reddish: both of them preserved their transparence, and at the point of contact of these two liquids, was perceived a concrete stratum, having several of the properties of camphor, but its quantity was so small that we were obliged to neglect it.

Mutual action of oil of turpentine and muriatic acid produces a small quantity of a concrete body.

A fact of this kind is to be met with in the translation of the Chemical Recreations of *Model*, published in 1774, by M. *Parmentier*.

Previous discovery of this fact.

This philosopher expresses himself as follows, in page 400 of the first volume of this translation, in the additions which he has made to it:

"A very curious and very interesting observation to natural philosophy and chemistry, is, that M. *Marges*, surgeon, in a work, the object of which is the examination and chemical analysis of different medicaments *, says, that he obtained, from the digestion of a mixture of fuming marine acid and oil of turpentine, small saline concretions, which at the end of a certain time, became more considerable, and took the figure of a crystalline salt, in form of a parallelopipedon, while the oil which swam above them was coloured red, and acquired a very thick consistence, &c.

It is very probable that these crystals were the same as those which were obtained in such great abundance from the action of the same gazeous acid.

The same oil was saturated with oxigenated muriatic acid gas;

Oxigenated muriatic acid gas and oil of turpentine.

* Paris, 1774. Second edition.

gas; it acquired a deep yellow, retained its tranſparence, became a little more conſiſtent, but did not produce any camphor.

No camphor.

Action of ſulphuric acid.

Concentrated ſulphuric acid acting too violently on oil of turpentine, the decompoſition of which it ſpeedily effects, we made a mixture of equal weights of this oil and ſulphuric acid marking only 40° of the areometer. The action was confined to the point of contact, and was analogous to that of liquid muriatic acid.

Slow diſtillation of oil of turpentine.

Eight ounces of oil of turpentine were kept for three days in a glaſs alembic, placed on a ſand-bath, the temperature of which was maintained at about forty degrees of *Reaumur's* ſcale. This time being elapſed, we found in the recipient nearly four ounces of a very white and very light volatile oil, and in the capital many ſmall cryſtallizations of true camphor. The oil remaining in the cucurbit was of an amber colour, and very much thickened.

Action of muriatic acid gas on the other volatile oils.

To ſatisfy ourſelves whether the other volatile oils would furniſh camphor by the action of the muriatic gas, we incorporated them with it in different proportions; but did not obtain any precipitation. They became very black, and contained a great quantity of heavy oil, &c.

CONCLUSION.

General reſults.

1ſt. That the production of camphor, announced by M. *Kind*, is uniform in the proportions he indicates, and of whatever ſoil the oil of turpentine may be.

2nd. That the camphoric product may be eſtimated at about half the quantity of the oil employed.

3rd. That a greater quantity of the gas does not add to the product, nor does it deſtroy it.

4th. That this camphor may be purified by water, by alkaline ſolutions, charcoal aſhes, quick-lime and alumine: and, that theſe three laſt ſubſtances are more eſpecially preferable.

5th. That it differs from what is extracted from the camphor-laurel, in its taſte, which is not ſo bitter, and in its odour, which is leſs penetrating; and, that it preſents an abſolute difference in its mode of action with the nitric and acetic acids, the firſt of which only diſſolves it by a reciprocal decompoſition.

6th.

6th. That, notwithſtanding theſe differences, it is not the leſs a beautiful new ſpecies of this immediate principle of vegetables to chemiſts; and, that it is to be wiſhed that phyſicians would try it, to aſcertain whether, as there is reaſon to believe, its action will be the ſame on the animal economy.

7th. That, during the operation from which this camphor reſults, there is formed a true combination of muriatic acid and volatile oil, having a reſemblance to what the ancients called *acid ſoaps*.

8th. That liquid muriatic acid, and dilute ſulphuric acid, not mixing with oil of turpentine, their action is confined to their point of contact.

9th. That the mode of action of the muriatic gas, in this circumſtance, may be explained by the elegant theory which MM. Fourcroy and Vauquelin have given of the action of ſulphuric acid on vegetable ſubſtances in general.

10th. That, notwithſtanding camphor is ſet free by the ſlow diſtillation of oil of turpentine, without addition, we are led to believe that ſo large a quantity of this matter could not exiſt ready formed in it, and that at leaſt a part is the reſult of the re-action of the principles of the oil, excited by the preſence of the acid gas, and its affinity for water.

11th. Finally, that the ſame proceſs is not applicable to the extraction of the camphor contained in the volatile oils of lavender and roſemary.

XVI.

Letter from Joseph Huddart, *Eſq. F. R. S. on the apparent Enlargement of the Moon at low Altitudes.*

To Mr. NICHOLSON.

Dear Sir,

It was not until yeſterday that I received your Journals, Nos. 36 and 37: In the former, reading a letter from C. L. occaſioned my referring to No. 35, page 164, of the Journal, for Mr. Walker's letter reſpecting the ſenſible magnitude of the Introduction.

the horizontal moon above what it has in the zenith; and therefore present you with the idea I have entertained upon the subject.

Estimate of magnitude confined to the size of the image on the retina.

I am of opinion that the magnitude of any object can only be estimated (as to sense) according to the space or magnitude of its image upon the retina. The eye must (if this is admitted), therefore, be the sole cause of the illusion, for the moon's apparent magnitude increases as she rises towards the zenith.

The eye is formed not only to alter its aperture, but also its focal distance.
Elongation of the eye will enlarge the image, though the angular magnitude remains unaltered.

The eye by the power of the muscles can, at the will of the observer, not only be directed towards the object, in order to receive its image upon the usual part of the retina, but acquire a proper convexity for distinct vision, or focus of the refracted rays upon the retina; and also to contract or extend the area of the pupil according to the quantity of light. This admirable property may be observed by every one in passing out of a light room in the night, and also in the eyes of the brute creation; but I conceive, that while the area of the pupil is enlarged in order to receive a greater quantity of light, the eye is protruded, or the focal distance between the pupil and retina is increased, which increases the sensible magnitude of the object, as the image upon the retina conveys it to our senses, when viewed by the naked eye. This I distinguish from apparent magnitude, which we use when actually measured by an instrument.

Variation of the pupil, or of the aperture of a lens, cannot alter the image.

The variation in the magnitude of the image upon the retina, certainly cannot arise from the variations in the dimensions of the pupil, which is contrary to the law of dioptrics; for, on observing the sun, I do not admit light through one-twentieth part of the area of the object glass, which is 3.8 inches in diameter, and yet there is not any difference in the apparent diameter; or, whether the rays are admitted at the center or any other part of the object-glass, the observations are equally good. But I consider it as a natural consequence, that when the pupil of the eye is enlarged, the focal distance is increased, and which constantly arises from a diminution of light, even without our attention; but thereby our sight is assisted in consequence with more light and power, which is some compensation.

The elongation of the eye is supposed to take place whenever the pupil is enlarged;

The light from a celestial object near the horizon, is diminished by passing a long distance through a gross atmosphere; hence

and as this takes place when the moon is least lu-

hence a fenfible enlargement takes place in the moon as well as other bodies, which diminifhes as the altitude increafes, and the light lefs interrupted.

minous, it then appears largeft.

I am, Sir,

Your moft obedient,

J. HUDDART.

Highbury Terrace, Jan. 28, 1805.

XVII.

A Memoir on Milk and the Lactic Acid. By Cit. Bouillon Lagrange *.

SECTION I.

The prefent State of our Knowledge refpecting the Lactic Acid.

SCHEELE and Deyeux, and Parmentier, are the chemifts who have moft contributed to our knowledge of the acid of milk.

History of experiments on milk, and the authors of difcoveries, &c.

The experiments made before the time of thefe fkilful philofophers, are thofe of Hoffman, Boerhaave, Homberg, Geoffroy, Rouelle the younger, and Baumé. But the analyfis of animal matters was then in fuch an imperfect ftate, that little can be gathered from their experiments.

Scheele directed his labours with very different views. His well conducted experiments led him to determine the characters and properties of an acid afforded by the ferum of milk But notwithftanding the labours of this celebrated man, we have fome fubjects of inquiry left for examination.

Scheele.

Befides the great number of its faline compounds, as Fourcroy remarks, which are ftill wanting to the fcience, Lehale has

Imperfect ftate of our knowledge of the lactic acid.

* Annales de Chemie, No. 150. vol. 50.

The author in a note mentions his having learned on the evening when his Memoir was read, that Meffrs. Vauquelin and Thenard had each been feparately employed on the fame fubject: and on that occafion he then finds its neceffary to fay, that moft of the experiments he defcribes, are the refult of obfervations made by feveral pharmacians, at the fitting of the fociety of pharmacy, the 15 Nivofe, in the year 12. The verbal procefs of that day, figned by Parmentier, prefident, and Delunel, fecretary, of which an extract was fent to the Philomathic Society, afcertains the experiments he had then made, and the obfervations of of the feveral members of the fociety of Pharmacy.

not

not pointed out the action of fire upon this acid, nor its spontaneous alteration in the air, the manner in which it comports itself with the nitric acid, &c. It is not known whether it be totally decomposed by this last, or converted into some other acid, particularly the oxalic acid. We are entirely unacquainted with the nature of its composition. Though it presents properties which resemble those of the acetous acid, and lead to the probability that it nearly resembles it; we cannot yet rank it with the vegetable acids. On the other hand, nothing decisive can be asserted with respect to its animal nature, because no experiment has yet indicated the presence of azote, and it is yet unknown, whether it may afford ammonia in its decomposition; whether it be putrescible, or convertible into prussic acid, &c.

This general statement while it gives us a view of our knowledge of the lactic acid traces, as it were, the steps necessary to be pursued in determining the place which this acid ought to occupy, as to the number of well established chemical facts.

SECTION II.

Concerning Milk and the caseous Matter.

On milk, and its constituent parts.

Before I proceed to describe the experiments I have made on this article, it will be useful to present some reflections on milk, and its constituent parts.

Guiton, in the Encyclopédie Methodique, offers two questions respecting the existence of an acid in milk. He expresses himself thus:

Whether whey exists ready formed in milk.

Does the whey exist in milk such as it is found after the separation of the other constituent parts? Does it manifest acid properties on any other account, than because it holds salts in solution, as all the analyses suppose?

Milk cannot be reproduced by the mixture of its parts.

If whey, adds the same chemist, existed in milk, in the state it exhibits after the separation of the butter and cheese, we should be able to reproduce milk by mixing these three principles again in the same proportions. But as this is not the case, he concludes, that whey is the product of a true fermentation.

The examination of this first question founded on experiment, ought, I think, to prove that it will not be sufficient to shew whether whey exists in milk, that we should mix the three principles in the same proportions, and re-produce that fluid; first, because these principles are no longer the same; and

and fecondly, becaufe whey formed by the ufual procefs, is more or lefs acid.

Experiment 1. Recent milk gives a red colour to the paper and the tincture of turnfole. Exp. 1. Milk reddens turnfole.

Experiment 2. If milk be diftilled in clofe veffels, and the product feparately taken, the firft is not acid; the fecond reddens turnfole, and flightly precipitates the nitrate of filver; and the third has no action on the tincture. At this period, the milk is not yet decompofed, but ftill reddens turnfole. It appears that the remaining acid is no longer volatile, but is retained either by the animal matter, or by fome other fubftance. Exp. 2. It affords acid by diftillation, and continues acid.

Experiment 3. If milk be coagulated with a mineral or vegetable acid, the whey is feparated, without retaining a particle of the acid made ufe of. Exp. 3. Whey of milk coagulated by an acid, retains none of the acid itfelf.

I muft however obferve, that fome deception would follow, if the ferum only were examined, which is obtained by the fulphuric acid, or by allum, becaufe the precipitate afforded by barytes, is not entirely re-diffolved by the nitric acid. I am fatisfied that this arifes only from a fmall quantity of fulphurate of potafh contained in the ferum, as is evidently proved, when the whey is feparated by any other fubftance not of an acid nature.

It is not true therefore as many chemifts have advanced, that the acid feparates the cafeous matter, by uniting with the ferum. Exp. 4. Whey from milk fpontaneoufly decompofed, is alfo acid.

Experiment 4. If milk be expofed to the air at the temperature between 12 and 20 degrees (about 66° Fahrenheit) the feparation takes place in twenty-four hours. The curd has a more acid flavour than that of experiment 3. Hot water caufes it to lofe its four tafte, and acquires the property of reddening turnfole.

We here find nearly the fame properties in the cheefy matter, when fpontaneoufly formed, and when feparated by acids.

Experiment 5. The fame experiment being made with the pneumatic apparatus, was attended with no abforption of air, or difengagement of elaftic fluid. The only difference was, that the feparation of the cheefy matter did not take place for feveral days. Part floated on the liquid, and the other occupied the bottom of the veffel. Exp. 5. Milk fpontaneoufly decompofed, neither abforbs nor gives out air.

Experiment 6. A bottle was filled with frefh milk, and well corked. Some days afterwards, the cheefy matter had feperated. As foon as the feparation was complete, the cork was intended to be extracted, but it flew out with violence and noife, Exp. 6. Milk decompofed fpont. in a clofed bottle, gives carbonic acid.

noife, indicating the prefence of an elaftic fluid. Upon fhaking the bottle, a greater quantity of gas was difengaged, which being collected and examined, had the fame characters as carbonic acid.

The fluid as well as the curd had a fharp acid tafte, which became lefs pungeut, as the carbonic acid was difengaged by agitation.

The fluid, after having been heated, did not appear to be more acid than ferum, obtained by expofing milk for the fame time to the air.

The fame experiment was made with milk which had been boiled about half an hour, and the refults were the fame.

Exp. 7. Qualities of the cheefy matters.

Experiment 7. The cheefy matters of the third, fourth, and fifth experiments, differ effentially in tafte and confiftence.

That of No. 3. is dry and firm; that of No. 4. is more divided, lefs dry, and partly foluble in water; and laftly, that obtained in the experiments No. 5 and 6. is not clotted, but is more light, and does not unite into a mafs, till after feveral hours.

The cheefy matter, therefore, requires particular properties, according to the fubftances and the proceffes employed to feparate the ferum.

Deductions from the facts.

Thefe firft experiments lead us to the following reflections, that, 1. Milk does not require to be decompofed, in order to manifeft the prefence of an acid. 2. That this acid is mixed with falts, fugar, and animal matter. 3. That the acid in milk is difengaged, though it is not very perceptible, but by re-agents. 4. That the contact of the air is not neceffary for feparating the conftituent parts of milk. 5. That the coagulating fubftances merely facilitate the feparation of the cheefe, either by forming a new compound, or by more immediately condenfing the particles together, when the cheefy matter requires new properties: or laftly, by expofing the milk to the air. By degrees, a part of the fugar of milk is decompofed; carbonic acid is formed, of which one part is difengaged, and the other facilitates the feparation of the curd; caloric alfo favours the attractions of coagulating matters for the curd. They all act differently, for alcohol, which alfo poffeffes this property, affords other refults; the curd being to a certain point foluble in water. We cannot therefore as chemifts have afcribed thefe effects to the folution of thofe fubftances in water, and their greater attraction for the liquid than is exerted by the cheefy matter itfelf.

(To be continued.)

Outlines of Pendulous measures of time
By J. Whittley Boswell Esq.r

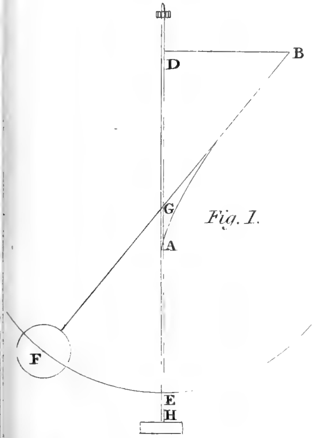


Fig. 1.

Fig. 4.

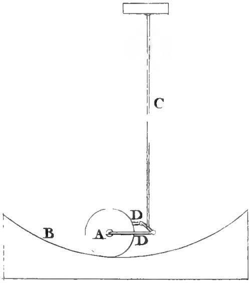


Fig. 2.

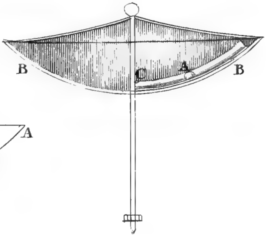


Fig. 3.

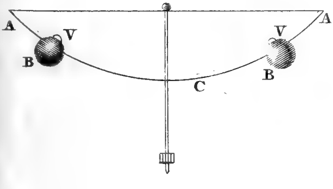
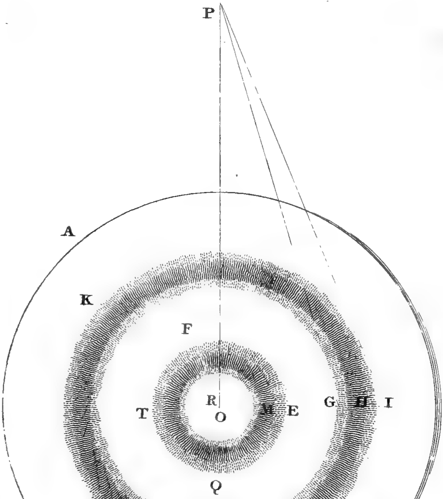


Fig. 5.

Mutlow Sc. Russell Co.t

Mr. Gough's theory of the Augmentation of Sound by communicated vibrations.



Mutlow Sc. Russell Ct.

Capt. Mortlock's Method of surveying, by very simple Instruments.

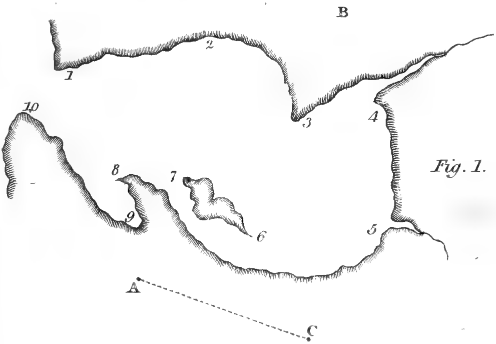


Fig. 1.

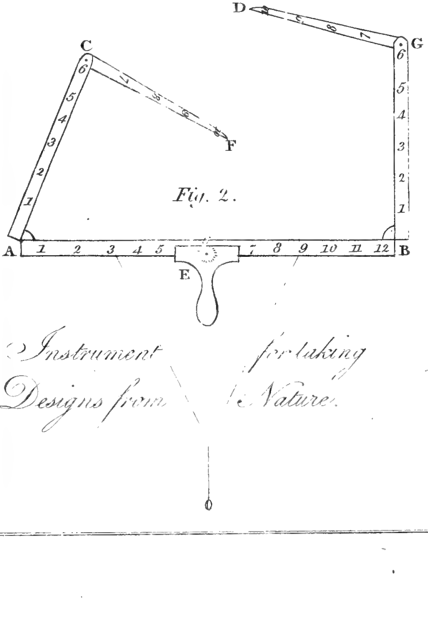


Fig. 2.

Instrument for taking Designs from Nature.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1805.

ARTICLE I.

Historical and Critical Observations relating chiefly to the Invention of the Telescope. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

ALTHOUGH I was aware that the experiment on the incompressibility of water was to be found in Lord Bacon's work, yet I was happy to see that Sir H. Englefield had taken the trouble of reminding your readers of the original inventor of it. I am always glad when I see any thing which may recall our attention to the works of our illustrious countryman. They abound with instruction, and, in many instances, his conjectures may be compared to the Queries of Sir I. Newton, which contain the foundations of the greatest discoveries. Incompressibility of water.

In the same paper Sir H. Englefield has given us a very curious passage from Kepler; and although I cannot persuade myself that he is accurate in his interpretation of it; yet I think the argument has been by no means accurately stated by Aletes *. I shall therefore beg your permission to insert some additional remarks on a subject, which certainly possesses a considerable share of interest. I must beg leave, likewise, pre- Remarks on the communications of Sir H. C. Englefield and Aletes.

* Vol. X. p. 92.

Obfervations relating to the invention of the telefcope.

previoufly to remark that Sir H. Englefield has not afferted that Kepler was the original inventor of the telefcope. The only inference which can be juftly drawn from his words, is that they were invented as early as the year 1607. No one, indeed, who had attentively read Kepler's dedication of his Dioptrice, could have entertained fuch an opinion. He precifely ftates * that fince "ad magnum cumulum inventionum hujus ultimi fæculi acceffiffet Arundo Dioptrica, nequaquam inter vulgares commemoranda machinationes, circaque eam, alii de palma primæ inventionis certarent, alii de perfectione inftrumenti fefe jactarent, ... Galilæus vero fuper ufu patefacto in perquirendis arcanis Aftronomicis fpeciociffimum triumphum ageret; . . . . Ego ductus honefta quadam æmulatione novum mathematicis campum aperui . . . . . caufarum lege geometrica demonftrandarum quibus . . . effectus innitereñtur."

Before the invention of telefcopes, aftronomical obfervations were fometimes made with tubes, but more generally with rods (regulæ) which had fights fixed at each end. Thefe inftruments were generally called Dioptra, and the apertures in the fights were called foramina dioptrorum, not perfpicilla. Ptolemy in his Almegift † defcribes the inftrument, which is generally known by the name of his Triquetrum. It had fights at each end, with apertures in them, which Georgius Trapezuntius, the tranflator, calls foramina: he mentions, indeed, no particular name for this part of the inftrument, but Theon in his commentary ‡ defcribes the conftruction of it to be fuch, that the whole moon would appear through the fights εν τη διοπτεια. Proclus Diadochus, in his Hypotypofis Aftromicarum Pofitionum,** mentions the Dioptra of Hipparchus, which was an inftrument of the fame kind: fee Riccioli Almegiftum Novum, vol. I. B. 3. C. 10. § 4. But we can have no further doubt upon the fubject, if we turn to Flamftead's Hiftoria Cæleftis, vol. III. p. 97. He there fays that "haud pauci

* Pp. 53, 54. Lond. 1653. † Book 5, chap. 12.

‡ P. 258. Bafil, 1538. Notwithftanding what Harduinus fays, I have no doubt, but that the Dioptra mentioned by Pliny in his Natural Hiftory, B. 2 C. 69. was an inftrument of this kind; but no defcription of it is given, and therefore I cannot argue upon it in this place.

** P. 399. Bafil, 1541.

obfervationum

obſervationum Tychonicarum errores ex nudorum Dioptrorum uſu neceſſarie conſequuntur." Now it is only neceſſary to examine the Aſtronomiæ Inſtauratæ Mechanica, and we ſhall ſee that almoſt all the inſtruments, which Tycho Brahe uſed, are fitted with ſights, which were ſome perforated with ſmall holes, and ſome divided by narrow ſlits: this part of the inſtrument he called *dioptra*, the perforations he called *foramina*, and the ſlits *rimulæ*.

Obſervations relating to the invention of the teleſcope.

I could quote further proof that theſe words were commonly uſed in the ſenſes which I have affixed to them, but it would be not only unneceſſary but tedious. It muſt, therefore, remain with Aletes to produce as good authorities for the alledged difference between *perſpicillum* and *perſpicillum vitreum*. I cannot help thinking, however, that the very title-page of Kepler's Dioptrice will be fatal to his explanation; for there we find that mention is made of the diſcoveries of Galilæo, made "*ope perſpicilli*" after the publication of the Siderius Nuntius. Here the word *perſpicillum* is undoubtedly uſed for the teleſcope itſelf, and before I conclude what I have to ſay, I ſhall quote a paſſage from Galilæo, which is ſtill more contradictory to the ideas of Aletes.

Although I differ from your correſpondent with reſpect to the arguments, which he has ſtated to you, ſtill I agree with him in his general concluſion, that Kepler did not mean a teleſcope by the word *perſpicilla*. We learn, indeed, from Borelli's book de vero inventore teleſcopii, that * one account makes the invention as early as the year 1590; but when we compare this with the depoſition of Sara Gædarda † and what is ſtated by Galilæo in (p. 10 of) his Sidereus Nuntius, it ſeems moſt probable that teleſcopes were not known before the year 1609. If Zachary Jauſens was acquainted with them before that period, he ſeems not to have publiſhed his diſcovery to the world. Now Kepler, in the place above quoted from the dedication of his Dioptrice, takes an opportunity of praiſing the invention in the higheſt terms, and as he did this, when it was public, it is probable that he would have done ſo ſtill more warmly, if he had been the firſt who publiſhed an account of obſervations made with them; but there is nothing of this kind in the paſſage alluded to in his book de cometis.

* P. 25. † p. 31. This makes the invention about 1611 or 1613, but "de certo præfixo tempore non potuit dicere."

Obfervations relating to the invention of the telefcope.

He was looking at the ftars with a friend on the bridge at Prague, and they obferved one in particular. "Vidi" * he fays, "ftellam fub urfa, majorem cæteris per perfpicilla intuitus, quæ æquale cæteris fixis lumen mihi fine perfpicillis diffundere videbatur." It is not likely that the perfon, who mentioned the firft ufe of fuch an inftrument as a telefcope, would confine himfelf to fo dry an account of the effects produced by it. But there is another argument, which may be drawn from the book itfelf, and that is, when Kepler is giving an account † of his obferving a comet in the year 1618, he ufes the word telefcopium and not perfpicillum. "6 Sept. jam nulla amplius cunda vifu nudo potuit obfervari et telefcopio infpectus fatis magnus apparuit." "Sept. 23. Planitudo major fine telefcopio, quam per illud." ‡

But what puts it beyond doubt that Kepler was not in poffeffion of a telefcope fo early as the year 1607, is that we hear of no great difcoveries which he made about that time by obferving the heavens. Monfieur de Moutucla, indeed, fays § that "Uniquement appliqué a determiner avec precifion les mouvemens celeftes, cet homme celebre faifoit peu d'ufage du telefcope." He probably thought himfelf juftified in this, when others were employéd in the more mechanical labour of obfervation. When he had no fuch fellow-labourers, it is impoffible that a man like Kepler could have neglected an opportunity of obferving, or have concealed the means of doing fo; and if he had not done both, we muft have had fome notices remaining either in his own writings or thofe of others.—— We muft recollect likewife, that he was the firft who underftood the theory of telefcopes and the combination of lenfes, and we muft not forget, that thefe difcoveries being once made, the greateft difficulty was overcome; it was only neceffary to ufe the telefcope and mark the objects which it prefented to the fight.

From what I have ftated there feems to be confiderable difficulties attending either of the ways, which have been propofed for interpreting the word perfpicilla. But there are fome paffages in the Sidereus Nuntius, which feem to affift us in affixing a meaning to the word, lefs exceptionable than the one hazarded by Aletes.

* $\frac{16}{26}$ Sept. 1607. † P. 48. ‡ P. 58.

§ Hiftorie des Mathematiques, vol. II. p. 234.

In

In p. 11. Galilæo fays " tubum primo plumbeum mihi paravi, in cujus extremitates vitrea duo perfpicilla . . . aptavi." Again in p. 12. he fays " dum nulla in tubo adeffent perfpicilla," the rays pafs in one direction, but " appofitis perfpicillis," they will pafs in another. Hence it appears that Galilæo makes no difference between *perfpicilla* and *vitrea perfpicilla*, but ufes both indifcriminately for the glaffes of his telefcopes. I am inclined indeed to believe that the original meaning of *perfpicillum* was like that of *confpicilium* a mere lens; that when lenfes were combined in telefcopes it was ufed both for the glafs and the inftrument, until the word *telefcopium* became common. I am not, however, prepared to bring forward fufficient authorities for this account of the word, and I have not leifure at prefent to fearch for them. It is fufficient for my purpofe that we are juftified in underftanding the word as meaning a mere lens; for if we confider it as fuch in the paffage under confideration all the difficulty vanifhes.—— Kepler complains in this very book *de cometis* * of the weaknefs of his fight; he might, therefore, have probably ufed a fimple lens to affift it. Monfieur Dutens † has clearly proved that the convex lens was known to the ancients, and at the fame time ‡ that Borelli calls Zachary Janfens, " confpiciliorum artifex peritiffimus," he fpeaks of his concave as well as convex glaffes.

Obfervations relating to the invention of the telefcope.

There is one circumftance attending the obfervation itfelf, which feems to give fome fupport to my interpretation, which is, that it was not made at a time when Kepler was carefully examining the heavens for any new phenomena; but in a walk which he accidentally took with his friend. Now he may eafily be fuppofed to have carried about with him a fimple lens to correct any fault of his eyes, but it is fcarcely probable that he would have had with him a telefcope, made, poffibly like Galilæo's, of a long leaden pipe. I fcarcely need add, that whatever clears and ftrengthens the fight will diminifh the apparent diameter of a fixed ftar, and confequently increafe the relative magnitude and light of the comet.

* P. 30. Inftrumenta Tychonica, &c. &c. et oculos vegetos requirunt, quæ omnia mihi defuerunt, 1607, and in p. 48. Debili fum vifu, 1618.

† Decouvertes des anciens. Partie 3, chap. 10. §. 273. ‡ P. 25.

I am

Obfervations relating to the invention of the telefcope.

I am confcious, Sir, that what I now fend you, is put together in a very hafty manner. Were my time at my own difpofal, I would endeavour to make it more fit to meet the public eye; but I am induced to fend you thefe hints in their prefent ftate, becaufe I rely as much upon the candour as on the learning and abilities of Sir H. Englefield. If I am miftaken, he will fet me right; and if I have any foundation for my opinion, he can eafily illuftrate my hypothefis by the paffages which agree with it, in the old writers on optics and aftronomy. I am, Sir,

Your obliged humble fervant,

E. O.

Oxford, Feb. 10, 1805.

P. S. Since I finifhed the letter, which I fent to you on the 10th inft. I have met with a paffage, which appears to me to decide the queftion on which I wrote to you. It is in Kepler's book, which he publifhed at Francfort in 1604, under the title of *Aftronomiæ pars optica.* The beginning of it is entitled *Paralipomena in Vitellionem.* He difcuffes the nature of the eye, and at p. 178. he begins a fet of propofitions on the manner in which it acts as the organ of vifion. The 28th of thefe propofitions is that which makes moft immediately for my prefent purpofe; it is at p. 200, and he enumerates it in the following words.

"Qui remota diftinctè vident, propinqua confufé; iis perfpicilla convexa profunt. Qui vero confufè vident remota, juvantur concavis perfpicillis."

The mere quotation of thefe words is fufficient for the proof of what I have afferted; but as it may afford fome amufement to your readers, I will add his remarks upon them as fhortly as poffible.—"Quanta admiratio" he fays, "rei tantæ tam late propogatum ufum: & tamen caufum ignorari hactenus." J. B. Porta, he adds, profeffes to give the reafon in his optics: but this book he was never able to meet with. Kepler then very ably refutes the opinion of thofe, who fuppofe this effect to be produced by the increafing or diminifhing of the apparent magnitude of the object. He offers with confiderable diffidence his own method of accounting for the phenomenon. He fuppofes that in the cafes mentioned in the propofition the vifion is imperfect, in as much as without the intervention

tervention of convex or concave lenfes, the apices of the different cones of rays would fall either before or behind the retina. He fupports his theory by experiments which he tried on fhort and long-fighted perfons; and quotes the paffage from Ariftotle, in which he points out the difference between the myops and the prefbytes.

Feb. 12.

II.

Conftruction of a Line in a Circle, nearly equal to the Side of a Square of the fame Superficies as that of the Circle itfelf. With Remarks on Pendulums and other Objects. By Mr. J. WHITLEY BOSWELL.

To Mr. NICHOLSON.

SIR, *Feb.* 9, 1805.

HAVING often found a difficulty in computing the preffure of fluids in tubes and cylindrical veffels, from all the calculations relative to the gravities of different bodies, which I was acquainted with, being adapted for cubic meafure, it appeared a very defirable matter to find an eafy method of converting round into fquare meafure, which induced me at different times to attempt it, and as often to lay it afide, from not being able to arrive at any fatisfactory refult. But lately having had occafion to refume this fubject, I have difcovered a method of coming fo very near the truth, that if it fhould turn out to be in reality not fo exact as it feems to me, it will ferve fo well for common computations, and promifes on that account to be of fuch utility, that I am induced to fend it to you for publication, if you approve of it. Introduction.

As this fubject has been by many looked on as fomewhat chimerical, it may be therefore not amifs to mention, for thofe of your readers who are not very converfant in geometrical ftudies, that a mechanical method of computing fquare meafure from round is a very ancient and well-known problem; but this method being difficult to apply to the infide of tubes, and not capable of being ufed in diagrams or drawings, has made another method defirable: mine for this purpofe is as follows:— To convert round meafure into fquare.

To

To find a right Line, the Square of which ſhall be equal to a given Circle.

Problem. To find by conſtruction the ſide of a ſquare equal to a circle.

Draw two diameters (I F and C T) biſecting each other at right angles in the center (O) of the given circle; biſect one of the radii ſo formed (O T) in W, and from the extremity (I) of the next adjoining radius, through W draw the right line (I B) to the circumference: This line (I B) is the line required.

Proof.

Proof by trial.

Let a ſquare be formed equal to the given circle by the third corol. prop. fifth, of Archimedes; then take a ſquare formed by the line I B, and place it on this other ſquare, ſo that one angle and the ſide adjacent to it of one, ſhall fall on one angle and the ſide adjacent to that angle of the other; then will it be ſeen that all the other angles and ſides of each will coincide, and the whole of one be equal to the whole of the other.

This kind of proof is nearly the ſame as that of the fourth prop. firſt book of Euclid, on which ſo many other propoſitions depend; and having often tried this method in the above manner, I could never perceive any difference between the two ſquares: In thoſe trials I uſed circles of card paper for the more exact meaſurement of the circumference in the mechanical proceſs directed in the method of Archimedes.*

To the above I have to add the farther proof which follows of the exactneſs of my method, which may make it appear ſtill more certain.

Sometime after I diſcovered the above, looking into a work of the learned Kircher for a ready method of deſcribing a pa-

* In order to ſhew how near Mr. B.'s conſtruction approaches to the truth, we may obſerve, that when the diameter is = 1, the area is = 0.7854, and the ſide of the equal ſquare = 0.8862. But in the figure, I W is found by adding the ſquare of the radius to that of the half radius, and extracting the ſquare root; and then by the property of ſimilar triangles, as the radius is to I W, ſo is the diameter to I B, which will be 0.8944 when the diameter is = 1. But this line exceeds 0.8862, or the true ſide of the ſquare, by 0.0082, or nearly one hundredth part. N.

4 rabola

rabola which I knew was inferted in it, I perceived therein alfo a geometrical method of finding a right line equal to any given portion of the circumference of a circle; and having by this method drawn a right line equal to the fourth of the circumference of a circle, and with it and the diameter formed a parallelogram equal to the circle, and then a fquare equal to this parallelogram, in the ufual manner: I afterwards formed another fquare equal to the fame circle, according to my method before defcribed, and found no perceivable difference between it and the former fquare.

The difcovery of a fact in geometry often leads to another; one of this kind I have to add here, which is, that a right line (B E) drawn from the extremity B of the line I B, at right angles through the oppofite diameter (I F) to the circumference, will be equal to a fourth of the circumference.

The problem of Kircher above mentioned, may be found in the *Ars magna Lucis et Umbræ*, prop. 3, lib. 3. pars 2, page 239, and the two following pages.

As the circles which I ufed were under three inches diameter, it is poffible on fo fmall a fcale a minute error might not be vifible; but even in this cafe it muft be very inconfiderable.

I take the opportunity of forwarding the above to fend an explanation in reply to the annotations on my paper on pendulums, which I hope you will do me the favour to infert, as you have in fome degree called on me for it. Properties of pendulums, &c.

I beg leave to obferve, that in that paper I have endeavoured to put a marked diftinction (though perhaps not fufficiently forcible) between the obfervations which admitted of proof, and fpeculations on new fubjects: The latter may be confidered to commence with Huygens' pendulum; and the others on fimilar principles, along with this, were merely defigned as objects *propofed for experiment*, not as things proved, and nothing is inferted about them but what was intended to be noted as conjectural by the mode of expreffion.

I confider it ufeful to propofe experiments (exclufive of my partiality to this mode of afcertaining facts), becaufe among your numerous readers there might be fome who, with little trouble or expence, have opportunities to make trials of this kind,

Properties of pendulums, &c.

kind, from the nature of their employments furnifhing them with all the materials neceffary ready at hand, and who have fufficient intereft in the fubject to induce them to undertake them.

I acknowledge that (as you obferve) I did not ftate all my reafons for thinking that Huygens' circular pendulum, and the others propofed on the fame principles, would not be fo liable to alter their notation of time, as thofe in common ufe, from a change of temperature; but this was caufed by the other fubjects contained in that paper having extended it to fuch a length as to oblige me to curtail this and other matters, and leave out part of what I had written relative to pyrometers alfo. My reafon for thinking well of Huygens' pendulum proceeded partly from my refpect for his opinion, founded on the high idea I have of his mathematical erudition; and becaufe it appeared to me, that the fize of the circle produced by the gyration of the ball, depending on the impulfe of the moving power more than on the length of the fufpending ftring; that if the ftring lengthened a little, the circle would not be thereby increafed; and even if it was increafed, that the effect of the paraboloidal lamina would prevent its altering the time of each revolution of the ball (granting the theorem of Huygens, which I inferted, to be true); for though I was aware that the parabola generated would, by the lengthening of the ftring, become of fomewhat lefs curvature, yet I thought this change would be fo minute as not fenfibly to affect the time, efpecially as a change apparently greater had not affected it in a fimilar cafe, which will be mentioned a little farther on, but of this I had doubts then, as appears from what is remarked in page 77, where I mention that I thought the other conftructions which I propofed might be better on account of *the ftring of that of Huygens being liable to lengthen.*

I recommended the other pendulums, on fimilar principles, for trial, becaufe though the expanfion will increafe the dimenfions of the parabolical and cycloidal curves, yet ftill they will not ceafe to be thofe curves, which Huygens has proved to have fuch remarkable properties for the regulation of pendulums; and as it has been found by experience, that a pendulum vibrating in a fmall arc of a large circle, has the fame accuracy as when moving in a cycloid, I imagined that there could not at leaft be more difference between the effects of

two

two cycloids or parabolas a little differing in fize, than between the cycloid and large circle; and befides, as the cycloidal cheecks ufed by Huygens (which he found to fucceed fo well) were liable to the fame alteration by expanfion, I looked on his numerous experiments as confirmations of this opinion.

Properties of pendulums, &c.

In reply to the obfervation relative to the fuperior effect of pendulums detached as much as poffible, I beg leave to obferve, that a movement interrupted or intermitting (fuch as more or lefs takes place in clocks with ofcillating pendulums), is fo very different from one which continues equally without any interruption (as that of the circular pendulum), that it does not feem to me conclufive to argue from the effects of one to thofe of the other.

In the note relative to the circular veffel enclofing the mercurial tube, I am inclined to imagine you were not aware that this veffel was directed to be faftened to the fpindle fo as to revolve with it, by which means there would be no lateral motion given to the air but by the friction of the outfide of the cafe; for which reafon I mentioned that it fhould be made very fmooth externally: I alfo directed it to be covered, to prevent the effect of the current of air, which would otherwife pafs through it, caufed by the centrifugal impulfe of the circular motion.

As to what I obferved about the fufpending fpring, I perhaps expreffed myfelf too generally: I ftill think, however, I could point out feveral inftances of gridiron pendulums, where the compenfations were made of one bar againft another only, without allowing any compenfation for this fpring.

I think it neceffary to mention among the emendations of my former paper, that the cycloidal bed of the rolling pendulum, propofed in it, fhould be formed of the fame thicknefs in every part, as it might expand unequally if made thicker in one part than another: As a trial of this fort of pendulum may be made with a common clock at a fmall expence, I hope it will induce fome one better fkilled in fuch experiments than I am, to make it, even if the others fhould not be tried: and as a farther reafon for expectation of fuccefs in the trial, it fhould be confidered (in addition to what has been advanced in fupport of the opinion, that the effect of the cycloidal bed would be but little altered by its expanfion), that,

Properties of pendulums, &c.

that, on account of the ſhortneſs of this bed, it could not poſſibly expand as much in length as the 39 inches of wire contained in a common pendulum-rod; and that, as the greateſt expanſion of the wire is certainly in the direction the moſt injurious to the effect of the pendulum, which is not the caſe with that of the cycloidal bed, it is probable that the rolling pendulum would be found ſuperior to that in common uſe.

With regard to the obſervations on pyrometers, I own I never ſaw that of Deluc; but as your deſcription of it is not ſufficiently minute to enable me to perceive how the ſtandard in it for meaſuring the different bars, could eſcape expanſion in an increaſe of temperature of the atmoſphere, I ſtill can conceive no method by which the ſtandard for meaſurement in any pyrometer, could be prevented from altering its length from the above cauſe, but by keeping it artificially at a fixed temperature, by ſome means diſtinct from the apparatus, ſimilar to thoſe of Mr. Ramſden, which you have mentioned. What I wrote on this ſubject was intended as a caution in future experiments, and if it has produced the more extended publication of means to avoid the error I pointed out, or ſhall hereafter do ſo, my view will be fully anſwered.

Your very humble ſervant,

J. WHITLEY BOSWELL.

III.

Some Remarks upon the Experiments by which Mr. Ez. Walker *has endeavoured to explain the apparent Enlargement of the Moon near the Horizon: with a Statement of ſome Facts upon which that Phenomenon ſeems chiefly to depend. In a Letter from* C. H.

To Mr. NICHOLSON.

SIR,

Mr. Walker's experiments adverted to.

I SHOULD, with your permiſſion, be glad to point out to your correſpondent, Mr. Ezekiel Walker, a theorem in optics, to which, it appears to me, that he has not paid ſufficient attention in the courſe of his ſtudies.

Rays

Rays from a diſtant object which paſs through a convex lens near its center, will have their focus more diſtant from the lens, than thoſe rays which paſs through it nearer to its circumference; and, conſequently, the image formed by the central rays, will be as much larger than that formed by the rays of the circumference, as the focal diſtance of the former ſhall exceed that of the latter. With a given aperture, the focus will be that point in which the greateſt number of rays coincide, for, *there*, will the image appear moſt diſtinct; and, conſequently, as we enlarge the aperture of the lens, the central rays muſt be combined with others more convergent, the new focus will approach the lens, and the image be, of courſe, proportionably diminiſhed.

Optical theorem. The focus of a lens is ſhorter the larger the aperture, and the image of courſe ſmaller.

The total diſagreement of this *theorem* with the reſult of Mr. W.'s experiments, I can only account for by ſuppoſing this gentleman, while he enlarged the aperture of his lens, not to have paid attention to the increaſing brightneſs of the image, which, by making a ſtronger impreſſion on the retina, would appear to increaſe in magnitude with every enlargement of the aperture. If I might be allowed to offer my advice to Mr. W. I ſhould recommend him to repeat his experiments, and ſubſtitute, for the flame of the candle, a ſkreen having a circular opening covered with thin paper. This, with a light behind it, may repreſent the horizontal moon. Some characters ſhould be marked upon it, in order to determine, with more facility, when the image is moſt diſtinct. The precaution of reducing the different images, as nearly as poſſible, to the ſame degree of brightneſs previous to meaſurement, muſt not be neglected. I am ſurprized indeed, that the ſtructure of the eye, which Mr. W. profeſſes to have kept in view in the courſe of theſe experiments, did not point out to him the importance of this circumſtance, which is ſo admirably anſwered, in the natural organ, by the contraction and expanſion of the pupil.

This is contrary to Mr. Walker's reſult; in which he was probably deceived by the differences in brightneſs.

Propoſal that he ſhould vary and repeat the experiment.

As the phenomenon of the horizontal moon has of late much engaged the attention of ſeveral of your correſpondents, I will avail myſelf of the preſent opportunity to add a few remarks on that ſubject. It appears, at firſt view, that the moſt obvious method of accounting for the phenomenon, is the following: We believe the moon more diſtant from us when in the horizon than when in the meridian; but we ſee it,

Phenomenon of the horizontal moon occaſioned in part by the ſuppoſition of greater diſtance, by the aerial perſpective, and other circumſtances;

it, in both cafes, under the fame apparent angle, and, according to our ufual habits of judging of the magnitudes of objects, neceffarily refer the idea of greater magnitude to the greater diftance. The aerial perfpective, and various other circumftances, which combine to make the horizontal diftance appear the greateft, have been fo well and fo often defcribed, that it would be fuperfluons to repeat them here. I muft obferve, however, that this appearance of greater diftance in the horizontal moon, though the firft and moft obvious circumftance which prefents itfelf to explain the illufion, is not, in my opinion, that which has the greateft fhare in producing it.

but thefe have not the greateft fhare in producing it.

After having confidered the fubject with much attention, I am of opinion that the chief ftrefs ought to be laid on the following particulars:

New explanation. The moon is rarely feen in the horizon.

We may be faid rarely to fee the moon in the horizon. Our habitual acquaintance with it is at fome confiderable altitude. Hence it follows, that all the objects with which it comes in apparent contact, and with which we occafionally compare its difk, are near to us, and fubtend angles proportionably large. Thus, for inftance, we often fee the moon lofe a large portion of her face behind the branch of a tree or a weather-cock, and totally difappear behind a chimney. The clouds, too, which fleet before her, appear, from their proximity to us, on a gigantic fcale. Thefe are the circumftances under which we have infenfibly formed our general ideas of the moon's magnitude. Let us now furvey it in the horizon. The cafe is widely altered. We have now an opportunity of comparing it with various large objects, which perhaps, being themfelves on the verge of a diftant horizon, will be reduced to very trifling dimenfions; and thus the caftle and maffy cathedral will hide but a fmall part of that globe, which we have commonly feen half eclipfed by a fpout or a weathercock. The clouds too, reduced by diftance, appear on a much fmaller fcale when compared with the moon. In fhort, it is no longer the fame moon we were acquainted with in the meridian, but a much larger and more majeftic fphere, the novel appearance of which ftrikes moft of its fpectators with a degree of awe and amazement.

Small objects eclipfe the moon at confiderable altitudes.

The elevated clouds are alfo large, becaufe near; and the moon feems fmall, becaufe compared with them.

But in or near the horizon, the clouds, buildings, and other large objects, will appear fmall, and hide but little of the moon's difc; which will therefore, by comparifon, be feen on a grand and enlarged fcale.

Though it appears to me that the circumftance I have pointed out, of our habitual comparifon of the moon with objects

Thefe are the principal circumftances.

objects of known magnitudes, has the greatest share in producing the illusion by which it appears largest in the horizon, I would by no means be understood to reject the principal circumstances, long since noticed as contributing towards the same effect. Different circumstances will necessarily have more or less influence with different persons, according to the tenor of their previously acquired habits and observations.

I remain, Sir,

Your obedient servant,

C. H.

Tavistock Place, Feb. 15.

ANNOTATION. W. N.

THIS very striking and perspicuous explanation of the enlargement of the moon near the horizon, will bring to the reader's recollection a great number of facts, where the absolute magnitudes of objects, of which the distances are imperfectly known, are erroneously estimated by comparison with other objects lying in the same direction. Navigators must have been often struck with the extreme minuteness of the image of the sun or moon, when brought down by Hadley's quadrant so as to be seen projected upon the near objects below the horizon. Since the perusal of this communication, I have repeated the experiment on those luminaries. If the image of the moon be removed a certain number of degrees from the direct ray, it is easy, by altering the position of the quadrant, to observe that image in the horizon to the right or left, or upon the pavement before us, or at a great elevation in the heavens. In these cases its apparent magnitude, while the angular size and brightness remain unaltered, is found to be small at the great elevation, large in the horizon, and most so when projected upon distant objects, and it becomes a minute speck when seen depressed among the objects close to the observer. The same experiment may be made with a pane of glass, but less objectionably, because it might be urged that the quantity of illumination is different according to the obliquity of reflection.

Experiment with Hadley's quadrant, in which the sun and moon appear of different magnitudes, according to the objects on which their images are projected.

A Mathe-

IV.

A Mathematical Theory of the Speaking Trumpet. By JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR,

Theory of the ſpeaking trumpet.

THE ſpeaking trumpet is a conical tube, which receives the human voice from the mouth, and encreaſes the range of it. The ſtructure of theſe inſtruments, and the materials of which they commonly conſiſt, teach us to refer this ſingular faculty to two cauſes. In the firſt place, the ærial pulſes undergo certain modifications in the cavity of the trumpet; the nature and effects of which I am going to inveſtigate. Secondly, the metallic ſhell of the inſtrument augments the power of the voice, by its aptitude to conduct thoſe impulſes; which are impreſſed upon it, partly by the pulſes of the included air, and partly by the direct action of the Larynx, tranſmitted through the medium of the face to the mouth-piece of the inſtrument. Before I enter upon the inveſtigation of the effects reſulting from the firſt cauſe, it will be neceſſary to premiſe the following lemma:

Article 1. Let O R S *r* (*Fig.* IV. *Plate* 7) be an evaneſcent ſphere of air, which is agitated by two or more pulſes, beginning and ending together, and radiating from the points P *p*, &c. the vibatory motion of O R S *r*, in any direction whatever is equal to the joint ſum of the forces, which the pulſes would impreſs upon it ſeparately. Join P O, O *p*; and draw the lines R O *r*, S O *s* perpendicular to P O, O*p*; now as theſe lines are evaneſcent, every point of each of them is equally preſſed at any inſtant by its reſpective pulſe. But the force imparted to R O *r* by P, agitates the ſphere O R S *r* equally in all directions, *Principia*, 422; the ſame holds true of the force of *p* upon S O *s*. Now theſe forces cannot coaleſce or be compounded ſo as to act in a ſingle direction,—*Manch. Mem.* V. 5. p. 660; conſequently they act with their joint powers in all directions.

Art. 2. This being premiſed, let the fruſtum A B C D (*Fig.* 3.) repreſent a trumpet, the ſhell of which is a non-conductor of ſound. After a pulſe of the voice has paſſed through the aperture

aperture A B, it is bounded in front by a fpherical furface, having O, the vertex of the cone D O C, for its centre, and fubtending the plane angle A O B; in other words, let fuch a furface be defcribed any where in the fruftum A B C D, and the variable denfity of any one particle of air in contact with it, will be equal to the cotemporary denfity of any other particle in the fame furface. For as foon as the motion is propagated into the internal air, the pulfe will expand as far as the fides of the tube will permit: *Princip.* 42.2: after which, the motion will be continued to the oppofite aperture C D, in right angles, 43 *ibid.* therefore it will be propagated along the lines A D, B C with equal celerities, fuppofe to T and V.—On the plane T V defcribe the furface T N V, in which the denfity of the air is every where equal at any inftant: Now every point of fuch a furface acts upon the air externally in contact with it, and is re-acted upon by the fame perpendicularly to itfelf, in fuch a manner that equal portions of the furface fupport equal degrees of preffure. Let U u be an evanefcent particle of the curve T N V, draw U K, $u\,k$ perpendicular to N O; then the fuperficial ring between the planes U K, $u\,k$ is as U K $\times$ U u; but this ring is as the preffure upon it; which is alfo inverfely as the radius of curvature at U, therefore if U K $\times$ U u be conftant, the radius of curvature is conftant; confequently it is equal to O T, and the truth of the propofition is manifeft.

Theory of the fpeaking trumpet.

Art. 3. Let f, be the force of a pulfe in the aperture A B; and let the internal air of the cone be agitated with an equal and fimilar impulfe at the point O; then the effect thus produced in the trumpet, will be the fame with that of the voice acting at A B, (by *Art.* 2.) But had the cone been out of the way, the fame force at the diftance O A, would have been uniformly diffufed over the furface of the hemifphere, having O for its centre.

Art. 4. Put A O$=r$; the variable diftance O T$=x$; the radius of an evanefcent circle$=d$; the verfed fine of half the angle A O B$=p$; 3.1416$=c$. Then as the force f is diffufed over the hemifphere whofe radius$=r$, in free air by *Art.* 3. that part of it which refides in an evanefcent point of the fame furface$=\frac{f\,d^2}{2\,r^2}$.

Theory of the ſpeaking trumpet.

Art. 5. The magnitude of the ſurface T N V $= 2 c p x^2$: but the force f is diffuſed over this ſegment by *Art.* 3. conſequently that part of it which is confined to an evaneſcent ſpace $q = \frac{f q}{2 c p x^2}$

Art. 6. Since the ſpherical ſurface T N V is convex to the plane C N D (*Art.* 2.) the agitation of the air in the latter will commence at the centre N, and extend thence by the application of ſucceſſive circles of the ſphere T N V to equal circles of the plane C D, having N for their centre; which operation will continue until T and V fall upon C and D.— Now as C N D is in contact with unconfined air, each point of it, upon being ſtruck, will become a centre, from which a pulſe will radiate freely. (*Princip.* 42.2.)

Art. 7. Let P be ſuch a phyſical point, and let its area $= q$; then if an evaneſcent plane $= q$ be drawn through P perpendicular to O P or x, the force impreſſed upon it by the trumpet will be $\frac{f q}{2 c p x^2}$ (*Art.* 5) but the ſame force is imparted at the ſame time to the equal phyſical point P in the plane (*Art.* 1.)

Art. 8. Let L be the place of an ear, in O N produced, or more properly of a minute ſphere of air. Put N P $= y$; P L $= w$. Now the pulſe proceeding from P will have due effect upon the point L (*Manch. Mem.* v. V. p. 662. cor. 1); which effect is as $\frac{f q}{2 c p x^2 w^2}$ (*Arts.* 6 and 7): hence we have (by *Art.* 4) as $\frac{f}{r^2} : \frac{f d^2}{2 r^2} :: \frac{f q}{2 c p x^2 w^2} : \frac{f q d^2}{4 c p x^2 w^2}$, for the force acting upon the ſphere L having d for its radius. But the number of points which act together $= \frac{2 c y y'}{q}$ (*Art.* 6.); conſequently their united forces $= \frac{2 f d^2 y y'}{4 p x^2 w^2}$. (*Art.* 1.)

Art. 9. Put L N $= g$; N O $= e$; O C $= a$; C L $= b$; and the correct fluent of the preceding expreſſion is $\frac{f d^2}{4 \times \overline{p g^2 - p e^2}}$ drawn into the hyp. log. of $\frac{g a}{e b}$; when g and e are unequal. But if C D biſect O L; put C N $= k$, and the correct fluent becomes $\frac{f d^2 k^2}{4 p g^2 b^2}$.

Art. 10.

Theory of the ſpeaking trumpet.

Art. 10. Though the foregoing fluents have received one correction, each of them requires a ſecond, ariſing from certain circumſtances peculiar to this problem. For it is evident, that the commencement of the agitation at N is prior to that at C, by a part of time, which is as $CO - ON$; conſequently the completion of the former pulſe at L, will precede the termination of the latter at the ſame point by a ſimilar interval; which is as $LC + CO - OL$. Put this time $= t$, and the duration of one vibration of the larynx $= l$; alſo let the indiviſable interval, or $\frac{1}{30}$ of a ſecond according to Euler be denoted by m; then multiply the expreſſions in *Art.* 9 by the fraction $\frac{m-t}{l}$.

Art. 11. The force determined in the preceding paragraphs, is that which a non-conducting tube imparts to an evaneſcent ſphere of air. This circumſtance affords a plauſible objection to the preſent theory; becauſe the ſenſe of hearing is not confined to a phyſical point. But as the ſeat of this ſenſation is of a given extent, at leaſt in each perſon, the number of theſe vibrating points, in contact with the ſenſitive ſurface, is alſo given: from this it follows, that the effect of one particle is nearly a true meaſure of the power of the whole number, when the ear lies at a great diſtance from the plane C N D, repreſenting the trumpet's mouth.

Art. 12. So much of the preſent ſheet is occupied by the conſideration of the firſt cauſe, that my remarks on the ſecond, or the effects of the metallic ſhell, muſt be confined within narrow limits; theſe I ſhall begin as follows.

Art. 13. Since the conducting power of the trumpet is given, the vibrations communicated to the ſmall end of it by the breath and face, will paſs to the oppoſite extremity in a given time; which call L. Alſo let h be the force of a vibration at the diſtance 1; and C L or b expreſs the ſpace betwixt the trumpet and a remote ear.

Art. 14. In the firſt place, let l be equal to L; then a ſingle pulſe of the trumpet will ſtrike the ear for every vibration of the voice; and the force of it will be $\frac{h\ m\ d^2}{L\ b^2}$ by my former paper on this ſubject. In the next place, let l be an aliquot part of L; then the number of pulſes impreſſed on the ear in

 the

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the time of L is $\frac{L}{l}$; hence the force $\frac{h\,m\,d^2}{l\,b^2}$. Add this equation to the former of thoſe found in *Art.* 9; and the ſum gives the force of the trumpet upon a diſtant ear.

Art. 15. But if l be an aliquant part of L, the ſound will be imperfectly formed; becauſe the quantity $\frac{L}{l}$ being a mixed number, the vibrations of the larynx will be conducted along the metal, and diſcharged into the air in a diſorderly manner, ſo as to deſtroy the continuity of the note. The ſame cauſe, in all probability, conſtitutes the difficulty which the inexperienced find in their firſt attempts to ſound horns and military trumpets: for the lips, which are compreſſed by the end of the tube, form the mouth piece in this caſe; and the art of producing the ſound ſeems to conſiſt in cauſing the edges of them to vibrate in aliquot parts of L, or the interval belonging to the loweſt note of the inſtrument. The ſame origin may alſo be aſcribed to the muſical ſcale, compoſed of what are called the trumpet notes; the intervals of which decreaſe in the following ratios, 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. for $\frac{1}{2}$ is the greateſt aliquot part of unity, $\frac{1}{3}$ the next in ſucceſſion, and ſo on.

Art. 16. If the remarks contained in the two preceding paragraphs be juſt, the perſon who makes uſe of a ſpeaking trumpet, muſt attend to the management of his voice; the pitch of which ought to coincide with ſome note in the trumpet ſcale of his inſtrument: for proper attention to this circumſtance will make the ſound of the metallic ſhell perfect, and bring it into uniſon with the other ſound; which ariſes from the ærial pulſes, paſſing through the cavity of the tube, at the ſame inſtant. In this manner, the greateſt ſound will be produced, ſuppoſing the power of the voice to be conſtant; becauſe the two ſets of pulſes, conſtituting the uniſon, will impreſs the greateſt poſſible force upon the ear in a given time. On the other hand, if the foregoing precaution be neglected, the imperfect ſound of the metal will diſturb the uniformity of the other, which is formed in the cavity; and the force impreſſed on the ear by ſo diſorderly an aggregate of pulſes, will be interrupted and inconſtant.

Art. 17. Perhaps it is unneceſſary to remark, that the perfect elaſticity of the air forms the baſis of the preſent theory; a ſuppoſition

fuppofition which in all probability is not altogether juft; but, when the fmallnefs of the angle O L C is recollected, the error arifing from the hypothefis vanifhes in a great meafure.

JOHN GOUGH.

Middlefhaw, Feb. 8, 1805.

V.

Obfervations on Bafalt, and on the Tranfition from the vitreous to the ftony Texture, which occurs in the gradual Refrigeration of melted Bafalt; with fome geological Remarks. In a Letter from GREGORY WATT, *Efq. to the Right Hon.* CHARLES GREVILLE, *V. P. R. S. From the Philofophical Tranfactions for* 1804, *p.* 279.

(Concluded from Page 126.)

A CURIOUS diverfity may prevail in the products of a compound body fubjected to fufion, when abfolute folution is produced. When merely fimple fufion takes place, the aggregation of the parts only is deftroyed: the fluidity arifes from the facility with which they move on each other; and a regulated diminution of temperature, by facilitating their re-union, can hardly fail to recompofe the fame fpecies that formerly appeared to exift in the compound. But, if the molecules have been diffolved and decompofed, and their component particles diffufed through the fluid, there feems to be very little probability that any re-union fhould compofe the fame molecules. It is more likely that new compounds will be formed, from which new molecules, and of courfe new cryftals, will be generated; and that, confequently, the fame rock may become the parent of very diverfified offspring. Thefe will however retain fome traces of their origin; for, as there can be no fufion of a compound body imagined, in which the mutual action of the components will not decompofe fome portion, there can be no folution fuppofed fo perfect that every molecule fhall be deftroyed. In the firft cafe, there will exift the germs of a new compofition; and, in the fecond, there will remain the relics of the old.

Fufion and cooling may produce different refults in compound bodies, the more powerful the one and gradual the other.

If thefe obfervations are correct, confiderable utility feems derivable from them, in the explanation of fome geological problems.

The aqueous formation exhibits fimilar facts.

problems. It will appear, that they ſtrikingly illuſtrate the analogy which exiſts between the aqueous and igneous formations, and ſhow that preciſely the ſame order and kind of arrangement is followed, in the generation of ſtony maſſes from water as from fire; for, the change of ſtructure, which I have obſerved to be the moſt inexplicable part of the proceſs by which glaſs paſſes into ſtone, is almoſt exactly imitated in the formation of calcareous ſtalactites. Succeſſive depoſitions of calcareous carbonate, form a ſtalactite, which at firſt is fibrous. A continuance of the proceſs cauſes the fibrous ſtructure to diſappear, and the ſtalactite becomes irregularly ſpathoſe. The irregularities then vaniſh, and it becomes perfect calcareous ſpar, diviſible into large rhomboids, with the form peculiar to that mineral; and all the gradations may be found in the ſame ſpecimen. Nor is this change confined to a few ſolitary ſpecimens; for a conſiderable extent of coaſt near Sunderland, is formed of a lime-ſtone compoſed of radiated ſpheroids, from half an inch to three inches diameter, imperfectly united. When one of theſe ſpheroids attains ſomething more than the uſual magnitude, it becomes compact in the heart; and it is not unuſual to diſcover portions of the rock, in which the radii have entirely diſappeared, and the whole maſs has become compact. It is probable that the entire formation of oolithi and piſolithi is owing to the ſame cauſe; and that they are prevented from ever arriving at great ſize, by the union of their ſurfaces, and their ſubſequent conſolidation into compact limeſtone, into which they are continually found to graduate.

Calcareous ſtalactites: at firſt fibrous, then irregularly ſpathoſe, then ſpar.

Coaſt near Sunderland ſo formed.

Hitherto, I have ſelected inſtances from ſubſtances which have an undiſputed claim to an aqueous origin. I ſhall now, on the authority of Dolomieu, inſtance a ſimilar arrangement, in a ſubſtance reſpecting the origin of which theoriſts are not agreed. A ſpecies of petroſilex is found in the Val de Nido, in Corſica, which contains radiated petroſiliceous glands, from half a line to an inch in diameter. Theſe glands only differ from the baſis by their radiated ſtructure, and their colour; and appear to indicate very clearly, that the rock was ſubjected to a ſpecies of arrangement which, if it had been completed, would have changed its nature, and probably would have rendered it porphyritic; for Dolomieu obſerves, that the centre of the glands was often occupied by a ſmall cryſtal of feldſpar.* The extra-

Petroſilex, of doubtful origin, ſimilarly arranged in its ſtructure.

* Dolomieu. Journal de Phyſique, 1794, page 260.

ordinary rock called the globular Granite of Corſica, is an analogous inſtance. It is compoſed of cryſtals of hornblende, feldſpar, quartz, and mica, in confuſed aggregation; and in this baſis are immerſed ſpheroids, about an inch and a half or two inches in diameter, compoſed of concentric alternate coats of quartz and hornblende. The centre is principally occupied by hornblende; this is ſurrounded by a zone of quartz. Theſe ſpheroids are radiated to the centre. There can be little doubt that this rock is merely the reſult of interrupted cryſtallization; and that, if the proceſs of arrangement had continued, this ſtructure would have diſappeared, and the whole rock would have reſembled the preſent baſis. Hitherto, this very ſingular rock has only been found in detached fragments.*

Globular granite at Corſica.

The admiſſion that ſolution is not a requiſite of cryſtallization, appears to me an important conceſſion in favour of the aqueous ſyſtem, which has laboured under very great embarraſſment, from the difficulty of diſſolving quartz. If a very perfect mechanical ſuſpenſion be all that is requiſite, we may

Solution not being indiſpenſable to cryſtallization, but only mechanical ſuſpenſion, many geological difficulties are removed.

* I ſhall venture to quote another inſtance, on the authority of Profeſſor Playfair. "The ſalt rock in Cheſhire, which lies in thick "beds, interpoſed between ſtrata of an argillaceous or marly ſtone, "and is itſelf mixed with a conſiderable portion of the ſame earth, "exhibits a very great peculiarity in its ſtructure. Though it "forms a maſs extremely compact, the ſalt is found to be arranged "in round maſſes, of five or ſix feet in diameter, not truly ſpherical, "but each compreſſed by thoſe that ſurround it, ſo as to have the "ſhape of an irregular polyhedron; theſe are formed of concentric "coats, diſtinguiſhable from each other by their colour, that is, "probably, by the greater or leſs quantity of earth which they con"tain; ſo that the roof of the mine, as it exhibits a horizontal ſec"tion of them, is divided into polygonal figures, each with a mul"titude of polygons without it, having altogether no inconſider"able reſemblance to a Moſaic pavement. In the triangular ſpaces "without the polygons, the ſalt is in coats, parallel to the ſides of "the polygons." Illuſtration of the Huttonian Theory, page 37.

I am informed, that the ſiliceous depoſition at Geyſer, is at firſt a porous friable maſs, and that the addition of more molecules renders it fibrous; alſo that, on a farther addition, the fibrous ſtructure diſappears, and the whole aſſumes the compact even texture of chalcedony or flint. If I am not miſinformed, a ſeries of ſpecimens illuſtrating this tranſition, exiſted in the cabinet of the late Dr. Hutton, of Edinburgh.

ceaſe

ceaſe to wonder at the almoſt daily formation of petrified wood, (in which, though cryſtallization does not actually take place, a very perfect arrangement is indicated, by the intimate union of the ſiliceous particles,) or of hydrophanous ſemi-opals in the decompoſed ſerpentine of Muſſinet, near Turin, or of chalcedony containing drops of water, in the decompoſed baſalt of Vicenza.

Since the cryſtallizing may not take place in the order of fuſibility, refractory ſubſtances may be impreſſed by ſuch as are more fuſible.

I have endeavoured to ſhow, that in the cryſtallizations reſulting from igneous fuſion, it is not only poſſible but probable, that the moſt infuſible ſubſtances might not be the firſt to cryſtallize; and this appears to involve important conſequences, for it partly removes one of the greateſt difficulties that embarraſſes the igneous theory, by explaining the poſſibility of refractory ſubſtances generated by fire being impreſſed by the forms of more fuſible ones. It ſeems, however, that the ſame order of arrangement would prevail in ſubſtances that were ſuſpended in a fluid medium, as the degrees of attraction would be the ſame. In either caſe, the firſt ſtep by which the arrangement of an apparently homogenous maſs commenced, would probably be the accumulation of particular molecules into little globules. Such ſeems to have happened in variolites, and other rocks which contain ſpherical concretions of a different nature from their baſis. Still farther advanced is the arrangement of porphyries: the molecules of one ſpecies have aſſumed a regular cryſtalline form; and ſometimes two or even more varieties o cryſtals are formed, which remain unmixed in the unarranged baſis. If the remaining molecules of that baſis are ſuſceptible of cryſtallization, it may be fairly concluded, that an extenſion of the proceſs of arrangement would convert the porphyry into granite, or at leaſt into one of the compound aggregates of cryſtals which conſtitute the numerous tribes of granites, grunſteins, and ſienites; and it ſeems equally probable that this might be accompliſhed, whether the molecules were indebted to a ſuitable temperature, or to an aqueous medium, for the requiſite facility of movement.

Inſtances and remarks.

Granites and porphyry.

The formation of granite and other rocks, muſt however be referred to the ultimate perfection of cryſtallization, by which all the molecules have been permitted to arrange. Thoſe granites called porphyritic, in which large cryſtals of feldſpar are imbedded in a baſis compounded of the ordinary ingredients of granite in ſmall grains, are apparently generated from a menſtruum

ſtruum in which the molecules of one ſpecies, being greatly predominant in number to the reſt, are the firſt to exerciſe their polarity, and conſtitute large cryſtals, which are afterwards ſurrounded by ſmaller ones, reſulting from the ſucceſſive ſeparations of the remaining elementary molecules.

The paſſage from the vitreous to the ſtony ſtate, from that to the porphyritic, and laſtly to the granitic ſtate are obſervable in lavas.

The changes of the ſubſtance that led to the foregoing remarks, ſerve to ſhow that they are not altogether hypothetical; and any proof that may appear deficient, ſeems to be provided by the phenomena exhibited by lavas, in which may be obſerved every ſtep of the paſſage from the vitreous to the ſtony, from that to the porphyritic, and finally to the granitic ſtate. The lava of Lipari, which paſſes from glaſs to lava, by the generation of minute globules, may be cited, on the authority of Spallanzani, as an inſtance of the commencement of the proceſs of arrangement; * and, were not their origin ſtill diſputed, I might alſo cite the pitchſtone lavas of the Euganean hills. It would appear, that the tranſition from the ſtony to the porphyrytic ſtate is rapid, for perfectly homogeneous lavas are among the rareſt of volcanic products. The porphyritic lavas are moſt numerous; and it is needleſs to detail the varieties they preſent.

* Spallanzani, *Viaggi alle due Sicilie. Tomo Secondo*, page 238. The whole paſſage, literally tranſlated, ſtands thus: "This lava "has a baſis of feldſpar, of a fine and compact grain, a ſplintery "fracture, rough to the touch, and emitting ſparks, like flint, "when ſtruck with ſteel. It has an aſh colour, in ſome places approaching to a leaden colour. It is thickly filled with an immenſity of little bodies, which would be diſtinguiſhed with difficulty, "from the reſemblance of their colour to that of their baſis, were "it not for their globular form. But this lava is joined to a great "maſs of glaſs, which forms a whole with it, without any diviſion "or ſeparation between them; and this lava, which in many places "retains its own nature, is in many other places reduced to glaſs. "Some parts of this glaſs are filled with the ſame little bodies, but "other parts are pure glaſs. This is in general very compact, has "a dead black colour, and breaks rather into irregular pieces than "into undulated fragments, as glaſs properly does. Beſides, it has "I know not what of unctuoſity to the touch, and to the eye, "which is not perceptible to the more perfect volcanic glaſſes. It "yields ſparks with ſteel, like the lava; but the lava is wholly "opaque, and the glaſs, at the angles and thin edges, has conſiderable tranſparency. It is only opaque where the globules are, "which appear to be particles of lava."

But,

But, though the proceſs of arrangement has often only advanced thus far, it has in many inſtances proceeded much farther, and it is by no means unuſual to find the entire baſis regularly arranged into cryſtalline bodies; thus, to cite a well known inſtance, in many of the ancient lavas of Somma, large augites are imbedded in a cryſtalline maſs, formed of minute cryſtals of leucite, together with another cryſtalline ſubſtance, whoſe nature is not perfectly determined.

Volcanic glaſs.

The caſual occurrence of volcanic glaſs is nowiſe at variance with this account, as it is ſufficiently probable, that ſome glaſſes may have a much greater tendency to cryſtalline arrangement than others poſſeſs; and it cannot appear extraordinary, that regular cryſtals ſhould ſometimes be generated, even in the glaſs, as it is a matter of daily occurrence in artificial glaſſes, and in furnace ſlags.

Peculiar bodies may be afforded by this method of ſeparation.

If the diſtinction attempted to be ſhown between igneous fuſion and ſolution be eſtabliſhed, it may offer a means of accounting for the abundance of peculiar bodies in lava, which do not exiſt in other ſituations, or at leaſt are of extremely rare occurrence. For, if the igneous action decompoſes the molecules of the ſubſtances on which it operates, there ſeems every probability that new compounds may reſult, diſſimilar to any ſubſtances we are acquainted with. It would appear, that the neceſſity of imagining an undiſcovered ſtratum abounding in leucites, chryſolites, and augites, may be diſpenſed with; and, as I have endeavoured to ſhow the probability that the moſt infuſible ſubſtances will not be the firſt to cryſtallize, the penetration of refractory leucites by fuſible augites, will ceaſe to be an argument againſt both being generated in the lava. I may alſo obſerve, that the ſame cauſes which vary the cryſtallized bodies reſulting from igneous ſolution, muſt operate upon the unarranged baſis; and that the ſame rock may be fuſed into lavas extremely diſſimilar, as their varieties muſt depend on the degree of ſolution which the fuſion has accompliſhed.*

The facts and doctrines here given will not affect the queſtion of the igneous or aqueous formation of baſalt.

If the analogy attempted to be ſhown between the aqueous and igneous formation appear founded, the tranſition from glaſs to

* The evidence of the generation of leucites in the lava which contains them, collected by Leopold de Buch, and Breiſlac, and finally acquieſced in by Dolomieu, appears ſo ſatisfactory, that it can hardly be deemed preſumptuous to aſſume the point as determined.

to ſtone can no way affect the great queſtion which has ſo long divided geologiſts, about the origin of baſalt; for though it is ſynthetically demonſtrated that baſalt may be formed by fire, the converſe of that propoſition ſtands ſupported by ſtrong analogical arguments, and its formation by water muſt be allowed to be at leaſt equally poſſible. How far the probabilities derived from the examination of baſaltic formations may influence the ultimate deciſion, is an enquiry in which I ſhall not now engage; though I cannot avoid recalling to my mind, the numerous inſtances of petrifactions found in baſalt, and, as a counterpoiſe to that obſervation, the equally numerous inſtances in which the heat emanating from it appears to have indurated ſtrata, and coaked beds of coal. One remark may be ſtated here with propriety, as it ariſes immediately from the experiment which has occaſioned theſe obſervations. In the ultimate reſult of that experiment, the arrangement of the molecules was much more perfect than in the original rock. It might be ſuppoſed, that a longer continuance of the ſuitable temperature was afforded it. This, however, could not be, for the maſs was only a few feet long, and a few inches thick; the fire was only maintained a day; and the whole was cooled in a week. But the hill of ſolid baſalt, from which the ſubſtance operated upon was taken, is ſeveral miles long, and ſeveral hundred feet high; and, ſuppoſing it to have been irrupted in a ſtate of igneous fuſion, it muſt have required months, nay years, for its refrigeration. How then comes it, that the proceſs of cryſtallization is ſo little advanced? How comes the confuſion of its texture to indicate the very reverſe of the tranquillity and perfection of arrangement, which may be fairly aſſumed as neceſſarily attending the extremely gradual changes of ſo immenſe a maſs?

Why ſo little of cryſtallization appears in the ſtructure of baſalt.

This objection admits of being obviated, upon the ſuppoſition that, in the proceſs of melting, the molecules of the baſalt were decompoſed; and that the new ones generated were more diſpoſed to cryſtallize than thoſe whoſe place they ſupplied. This explanation is in ſome degree juſtified, by the

Probably becauſe new molecules were formed more diſpoſed to cryſtallize.

mined. Neither de Buch nor Dolomieu have been able to convince themſelves that the augites were alſo formed in the lava; but I confeſs myſelf entirely unable to appreciate the cogency of their arguments, which ſeem annihilated by the admiſſion they have made in favour of leucites.

total

total diſappearance of the minute fieldſpar and hornblende of of the baſalt; inſtead of which, the regenerated ſtone contains thin laminæ of cryſtals, which are probably augites.

The diviſion of baſalt into globular maſſes during its decompoſition, indicates the formation of radiated ſpheroids therein;

I cannot leave this ſubject without noticing ſome particulars, in which the proceſs of arrangement deſcribed in the early part of this letter, appears to yield a probable explanation of ſome of the peculiarities of baſalt. The general diſpoſition of baſalt to divide into globular maſſes, in decompoſing, is too remarkable a fact to have eſcaped the attention of naturaliſts; though, as far as I am informed, no ſatisfactory explication of it has been given. The common effects of decompoſition are obviouſly inadequate; for it is common to ſee a large block of amorphus baſalt ſeparate into numerous balls, after a few months or years expoſure to the weather; and, rapid as the proceſs of decompoſition has been in the intervening portions, theſe balls reſiſt its father progreſs with uncommon obſtinacy. May not this be attributed to the formation of the radiated ſpheroids, whoſe occurrence in my experiment I have already mentioned? and may not their greater reſiſtance of weather ſimply ariſe from their aggregation being more perfect than that of the incoherent molecules which have filled the intervals between them? Though the radiated ſtructure has diſappeared to the eye, theſe portions of the ſtone retain the ſuperiority of more perfect internal arrangement; and, if my pigmy experiments could yield ſpheroids of two inches diameter, there can be no difficulty in ſuppoſing that the grand operations of nature may produce them of ſeveral feet. The ſeparation of the decompoſed fragments in concentric coats, ſeems eaſily explained; for I have already pointed out the facility with which the radii of the ſpheroids ſeparated at nearly the ſame diſtances from their centres, and the form of the fragments which reſulted, reſembling fragments of bombs. *

and theſe may have naturally formed the baſaltic columns.

If this idea be not conſidered as entirely diveſted of plauſibility, I may venture to extend the ſame principle, to account for the wonderful regularity of the priſmatic configuration of baſaltic

* Even granite has been frequently obſerved to affect globular decompoſition, and diviſion into fragments of concentric coats. This mode of decompoſition extends to many ſubſtances, that Werner has called the formation it ſeems to indicate, "*abgeſonderte ſtücke*," which has been rendered in Engliſh, *diſtinct concretions.*

basaltic columns, and also for their articulations. If we suppose that a mass of fluid basalt has filled a valley to an indefinite depth and extent, the process of arrangement in its particles must be induced by the removal of its heat or moisture, according as its solution is igneous or aqueous. This can only be done by the action of the atmosphere on its upper surface, and by the ground on which it reposes absorbing the heat or moisture from its under surface. From the variations of the atmosphere, its action must be irregular; and, from the perpetual change of the parts in contact with the heated or moist surface, its operations will always be nearly as active as at first, allowance being made for its variations. But the absorption of the ground will be regular, and regularly diminishing in activity, in proportion as the parts near the mass approach nearer to the same temperature, or same moisture, with the mass above; and its absorptions can only be carried on by its transmission of heat or moisture to the solid rocks below. From these considerations it seems evident, that the arrangement of the part of the basalt near the ground, will be begun with more energy than it can be continued, and that the results will be more slow and regular than the arrangement induced by the perpetual though variable action of the atmosphere. After the first stage in the process of arrangement has been performed, and a stratum, if I may so term it, of the jaspedeous substance extended over the surface of the ground, there seems no reason to doubt that a number of radiated spheroids would be generated in it, which would probably have all their centres about the same distance from the ground; and, as the arranging power undergoes a gradual diminution of energy, it is not probable that two rows in height of them should be formed at once, as that would indicate a hasty process, which had prepared a greater mass of matter for their almost simultaneous formation. From these considerations, there seems no improbability in supposing, that in the arrangement of a mass of fluid basalt, a single layer of radiated spheroids would be formed, reposing on the ground which supported the mass.

Enumeration of facts and circumstances.

1. Radiated spheroids formed near the ground, in one layer.

I have already stated, that when the radii of two spheroids came into contact, no penetration ensued, but the two bodies became mutually compressed, and separated by a plane, well defined, and invested with a rusty colour. I also stated, that when several spheroids encountered, they formed one another into

2. These would take hexagonal peripheries at the places of side contact;

into prifms with well defined angles. In a ftratum compofed of an indefinite number in fuperficial extent, but only one in height, of impenetrable fpheroids, with nearly equidiftant centres, if their peripheries fhould come in contact on the fame plane, it feems obvious that their mutual action would form them into hexagons; and, if thefe were refifted below, and there was no oppofing caufe above them, it feems equally clear, that they would extend their dimenfions upwards, and thus form hexagonal prifms, whofe length might be indefinitely greater than their diameters. The farther the extremities of the radii were removed from the centre, the nearer would be their approach to parallelifm; and the ftructure would be finally propagated by nearly parallel fibres, ftill keeping within the limits of the hexagonal prifm with which their incipient formation commenced; and the prifms might thus fhoot to an indefinite length into the undifturbed central mafs of the fluid, till their ftructure was deranged by the fuperior influence of a counteracting caufe, which would be provided by the action of the atmofphere on the upper furface of the bafalt. If this arrangement exifted, the fame caufe that determined the concentric fractures of the fibres of the fperoids, would produce convex articulations in the lower joints of the prifms; and, in proportion as the centre from which they were generated became more remote, the articulations would approximate to planes. If the generating centres were not equidiftant, the forms of the pillars would be irregular; and the irregularity would be in proportion to the diverfity of diftance between the centres. If the difference was great, the number of fides would be altered, and they might be found pentagonal, tetrahedral, and trihedral. As the compreffion of the fibres would be greateft in the level of the generating centres, the lower part of the prifms would be moft compact.

and other fpheroids formed above them would produce articulated columns.

The facts attending the production of bafaltic columns agree with the foregoing.

All thefe conditions feem to be fulfilled, in the actual conformation of bafaltic columns; for, in every inftance I am acquainted with, they appear to have been formed in the tranquil bofom of the mafs, as they have been originally mafked by amorphous trap, and their prifmatic ftructure is only difplayed by the removal of this covering. This has been varioufly effected, fometimes by the apparent difrupture of rocks, fometimes by the exterior portions of the mafs being thrown

thrown down by the failure of the ground on which it ftood, fometimes by the violence of the waves, and not unfrequently by the working of quarries. In moft inftances, thefe operations have only removed the covering from one fide of the colonnade; and it remains crowned, and generally furrounded, by an immenfe amorphous mafs. Where there are two ranges of columns, with an intervening amorphus ftratum, it is probable that the upper is the refult of a fecond inundation of fluid bafalt. It is well known that bafaltic columns are moft folid at the bottom; and their convex articulations have been repeatedly obferved. Since thefe confiderations occurred to me, I have had no opportunity of examining, whether the divifions approach nearer to the plane furfaces as they recede from the centre from which the prifms was generated, nor whether below that centre the convex furface of the articulations is inverted; but I think it by no means improbable, that fubfequent obfervations may eftablifh this to be the cafe, and thus confer on this hypothefis nearly all the demonftration of which it is fufceptible. I may however add, that the phenomena of prifmatic divifion in bafaltic veins, perfectly coincide with what might be inferred from the data upon which my reafoning has proceeded. In veins, it is obvious that the refrigerating or abforbing caufe muft operate with nearly equal force on each fide of the vein; and it follows, that two fets of prifms would be generated, which would be horizontal inftead of perpendicular, and that, unlefs a mafs of amorphous bafalt was interpofed between them, they muft form a divifion in the middle of the vein, as, from the mutual impenetrability of their fibres, they could not incorporate. The coincidence of the exifting phenomena with thefe conclufions, is fufficiently remarkable; for, in numerous obfervations I have made on the bafaltic veins which effect the prifmatic configuration, I found the prifms were always horizontal, and often, that there were two ranges of them. One of their ends applied to the wall of the veins, the other frequently united to an amorphous mafs which feparated them; and, when no fuch intermedium occurred, there was invariably a divifion in the middle of the vein. Not unfrequently, the veins contain three fets of prifms; a range of fmall ones on each fide, and of much larger ones in the middle. In this cafe, the little prifms are

Bafaltic veins.

are always feparated from the large ones, and the divifions of the large ones are very irregular. *

These deductions do not fuppofe either the aqueous or igneous formation of bafaltes to be eftablifhed.

After the ftatement of my opinion, that perfect fimilarity of ftructure may exift in the products of aqueous and igneous formation, it will hardly be neceffary to conclude thefe obfervations with remarking, that I fhould not confider the eftablifhment of thefe peculiar modes of arrangement as the flighteft demonftration of the igneous origin of bafalt. It appears to me, that the truth of my deductions is entirely independent of either theory, and that, if ever the period fhould arrive when the origin of bafalt fhall be determined by irrefragable demonftration, the inferences I have drawn may be accommodated with equal facility to either mode of agency. †

The

* The obfervations alluded to were made during the courfe of laft fummer (1803), on the very numerous bafalt veins, or, as they are there called, Whin Dykes, which traverfe the red fandftone and red fandftone breccia, which forms the greateft part of the coaft of the Firth of Clyde, between Greenock and the Largs.

† Mr. Keir, in his paper on the cryftallizations formed in glafs, fuggefts the probability of bafaltic pillars being formed by the cryftallization of vitreous lavas. See Philofophical Tranfactions for 1776, Vol. LXVI. page 530.

Dolomieu was of opinion, that the prifmatic form was peculiar to lavas which had flowed into the fea; and he attributed it to the fhrinking of the mafs: his defcription of the appearances exhibited by what he calls the prifmatic lavas at the foot of Etna, merits quotation.

" In the lavas of Etna, the form and dimenfions of the columns vary as much as the manner in which they are grouped; hexaedral and pentaedral prifms are moft abundant; then the tetraedral, the triedral, heptaedral, and octaedral. The leaft I have feen are only four inches diameter; others are more than three feet; they are commonly of a fingle fhoot, which is fometimes 60 feet high; others are divided by articulations, which are from one to fix feet afunder.

" I have more than once obferved a large column divide into feveral fmaller in its upper part. The columns are generally larger near the top than the bottom of the ftream of lava, becaufe they fubdivide; and they are always leaft in that part of the ftream of lava which firft entered the water, the refrigeration being more prompt,

Other rocks alſo affect the columnar form.

The immenſe magnitude of ſome baſaltic columns, the extreme regularity of their priſmatic configuration, and the peculiar ſtructure of their articulations, have directed the attention of naturaliſts to them, much more than to any of the other rocks which affect the columnar form. Yet many of theſe are ſufficiently remarkable to deſerve more particular notice than has generally been paid to them; and they afford moſt illuſtrative proof, that this configuration is not confined to either the aqueous or igneous formation; for ſome lavas, univerſally allowed to be ſuch, are priſmatic.* Columns of porphyry

prompt, and its effects more marked. Sometimes the columns are placed perpendicularly ſide by ſide, and form vertical walls, which are ſometimes more than 100 feet high, and a league long; ſometimes they are heaped obliquely, horizontally, and in all poſitions. Some, without being divided in their length, are larger at one end than the other; and then they are arranged like wood piled up, with all the ſmall ends at one ſide; ſometimes they are formed into pyramidal bundles, by parting from a common center; and, finally, there are ſome which, by their reunion, form large balls. Theſe radii of lava, which are rather pyramidal than priſmatic, reſemble thoſe of the globular pyrites, ſtriated from the center to the circumference, which are found in the chalk of Champagne.

" On the ſhore of la Trezza, near the mole, there is a very curious group of little articulated priſms, which iſſue from a common center, and form faſciculi ſingularly twiſted. The articulations are marked, but the ſpecies of vertebræ do not ſeparate. In the heart of the mountain on which ſtands the Caſtello di Jaci, there are large balls, from two to four feet in diameter, reſembling in form the large pyrites in the chalk of Champagne. Theſe balls of lava are formed of pyramidal columns, united by their points in a common center." *Catalogue des Laves de l'Etna*, page 453.

The diviſion of the upper part of baſaltic columns into ſeveral ſmaller ones, has alſo been obſerved in the baſaltic columns of Fairhead, by Dr. Richardſon. See Nicholſon's Journal, 4to. Vol. V. page 321.

* Almoſt all the priſms at the foot of Etna, deſcribed by Dolomieu, are of dubious origin; moſt of them are probably baſalt. The columns of the Vincentine are of the ſame ſubſtance, and ſo are the priſmatic lavas of Auvergne, and of the Vivarais. The bed of lava at la Scala, near Portici, is divided by vertical fiſſures, which give it the aſpect of irregular columns. At Aquapendente, in a quarry of undoubted lava, near the road, are ſome much more perfect

Inftances: Porphyry, petrofilex, argillaceous fchiftus, rubble flate, limeftone, &c.

porphyry are not rare; and, among other places, are found near Drefden, feveral feet in length, and not more than two inches in diameter. Columns of petrofilex compofe a large portion of a mountain near Coniftone lake. Very perfect quadrangular prifms of argillaceous fchiftus are found near Llanwrft. Rubble flate affumes the columnar form at Barmouth. The limeftone near Cyfartha, in Glamorganfhire, is divided into very regular acute rhomboidal prifms: even the fandftone of the fame diftrict is not unfrequently columnar; and one of the beds of gypfum at Montmartre is diftinctly divided into pretty regular columns. Sandftone, clay, argillaceous iron ore, and many other fubftances, become prifmatic by torrefaction; and the prifms of ftarch formed in drying have often been confidered as illuftrative of bafaltic formations.

Mere contraction may produce effects refembling thefe, but lefs regular.

I am very far from conceiving, that all thefe configurations are influenced by fuch fyftematic arrangements as have determined the form of fome bafaltic columns. I confider moft of them as folely attributable to contraction; which is only a farther extenfion of the aggregative force, and muft be regulated by the texture, the form, and the pofition of the mafs. Where the texture of the mafs is homogeneous, and its contractions uniform, its dimenfions may be diminifhed, without its continuity being deftroyed, provided its aggregation be fo ftrong as to overcome the *vis inertiæ* of the mafs, and its adhefion to other fubftances. But, when the refiftance is fufficient to overcome the aggregation, the mafs will be rent by fiffures perpendicular to the direction in which the greateft refiftance to its contraction takes place, or, in other words, by fiffures perpendicular to its greateft furface: for it is from the extremities of the greateft furface, that the largeft quantity of

Developement of the effects of contraction in producing fymmetric fractures.

perfect prifms; but the moft beautiful I have feen, are the fmall ones from Ponza. The columns at Bolfena are faid to be bafalt. Thofe of the Euganean hills are very irregular in form; in their texture they are certainly wholly unlike granite, which Mr. Strange thought they refembled. I believe them to be lava.

The mention of fome columnar formations that follows, is by no means intended as an enumeration of them. I have confined myfelf to thofe which I have either infpected in their natural fituation, or of which I have feen numerous fpecimens.

matter muſt traverſe the greateſt ſpace, in order that the contraction may be performed without breach of continuity; therefore, if it be an extenſive tabular maſs, it will be divided into priſms, by fiſſures perpendicular to its ſurfaces. The power of aggregation would determine theſe priſms to be hexagonal, as that form contains the greateſt quantity of matter in the leaſt ſurface, of any priſms that can be united without interpoſing priſms of other forms. But this would require the texture, the contraction, the thickneſs of the maſs, and its adheſion to ſurrounding ſubſtances, to be every where preciſely the ſame; and, as theſe conditions can never be fulfilled in an extenſive formation, all the irregularities that are found muſt neceſſarily enſue. The ſame rule that determines the fiſſures of a tabular maſs to be perpendicular to its ſurfaces, muſt determine the rents in a ſpheroid to be directed from its periphery to its centre.

They do not account for the regular form of baſaltes.

Though theſe conſiderations may be ſufficient to explain the tendency to diviſion into priſms, which is ſo generally extended, and which has produced many of thoſe abortions that have been dignified with the name of columns, becauſe they have occurred in lavas and in rocks of trap formation, they are utterly inadequate to illuſtrating the formation of the more perfect baſaltic priſms: they offer no means of accounting for the extreme regularity of the ſides and the preciſion of the angles, for the articulations, for the cloſe contact in which the perfect columns are placed to one another, not for their mutual adheſion, which is ſo ſtrong, that it often requires conſiderable violence to ſeparate them. Theſe facts are in abſolute contradiction to all idea of retreat or contraction, and ſeem to me to coincide perfectly with the explanation of their origin which I have already preſumed to lay before you.

I have the honour to be, &c.

GREGORY WATT.

VI.

Improved Construction of WOULFE's *Apparatus.* *By* W. B.

To Mr. NICHOLSON.

SIR,

Improvement in Woulfe's apparatus.

EVERY one who has been in the habit of using a Woulfe's apparatus, must have experienced the difficulty of fixing the tubes of communication, and the continual risk of breaking them. To obviate this inconvenience, I would suggest the following mode of constructing the apparatus, which, if you think any improvement upon the present method, cannot be rendered more generally useful than by insertion in your Journal.

I am, Sir,

Your very obedient humble servant,

W. B.

Feb. 5, 1805.

Fig. 1 represents one of the bottles. The bent tube, A B C, being attached to the outside of the bottle when both heated, and entering it at C, may be made as strong as is thought consistent with neatness.

The part H B of the tube is of a conical form, small enough at A to be inserted in a perforated cork.

The short tube D E, on the opposite side, is also of a conical form, large enough at E to admit a perforated cork.

The opening at F being intended for the tube of safety.

Fig. 2 represents the apparatus put together, the receiver being added as of a convenient form for connecting with the other vessels.

The small tube A B of one bottle, is inserted in the perforated cork fitted to the tube D E of the preceding one, and the joinings secured in the usual way by luting.

The tubes of safety being first securely fixed, and the cork closely fitted in the tube D E, any number of bottles may be successively added, without the least risk of deranging those that are already connected.

Observation

VII.

Obſervations on the Change of ſome of the proximate Principles of Vegetables into Bitumen; with Analytical Experiments on a peculiar Subſtance which is found with the Bovey Coal. By CHARLES HATCHETT, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

§ I.

ONE of the moſt inſtructive and important parts of geology, is the ſtudy of the ſpontaneous alterations by which bodies formerly appertaining to the organized kingdoms of nature have, after the loſs of the vital principle, become gradually converted into foſſil ſubſtances. Converſion of organized bodies into foſſils.

In ſome caſes, this converſion has been ſo complete, as to deſtroy all traces of previous organic arrangement; but, in others, the original texture and form have been more or leſs preſerved, although the ſubſtances retaining this texture, and exhibiting theſe forms, are often decidedly of a mineral nature. Some, however, of theſe extraneous foſſils (as they are called) retain part of their original ſubſtance or principles, whilſt others can only be regarded as caſts or impreſſions. The degrees are various.

From the animal kingdom we may ſelect, as examples, the foſſil ivory, which retains its cartilage *; the bones in Gibraltar rock, conſiſting of little more than the earthy part or phoſphate of lime; the ſhells forming the lumachella of Bleyberg, which ſtill poſſeſs the luſtre and irideſcence of their original nacre; and the ſhells found at Hordwell in Hampſhire, and in Picardy, which are chiefly porcellaneous, but more or leſs calcined; alſo the foſſil echini and others, ſo commonly found in the limeſtone, chalk, and calcareous grit of this iſland, which, although they retain their original figure, are intirely, or at leaſt externally, formed of calcareous ſpar, incruſting a nucleus of flint or chalcedony. And if, in addition to theſe, we may be allowed to regard the more recent limeſtone and chalk ſtrata as having been principally or partly formed from the detritus of animal exuviæ, we ſhall poſſeſs a complete ſeries of gradations, commencing with animal ſubſtances analogous in Inſtances in the animal kingdom,

* I have alſo found the cartilage perfect, in the teeth of the mammoth.

properties

properties to thofe which are recent, and terminating in bodies decidedly mineral, in which all veftiges of organization have been completely deftroyed.

and in the vegetable.

The vegetable kingdom has likewife produced many inftances not lefs remarkable; and it is worthy of notice, that animal petrifactions are commonly of a calcareous nature, while, on the contrary, the vegetable petrifactions are generally filiceous *.

It is not, however, my intention here to enter into a minute difcuffion concerning the formation of thefe extraneous foffils; I fhall therefore proceed to confider other equally or perhaps more important changes, which organized bodies, efpecially vegetables, appear to have fuffered, (after the extinction of the principle of life,) by being long buried in earthy ftrata, and by being thus expofed to the effects of mineral agents.

§ II.

Bituminous matters are derived from organized matters; chiefly vegetables.

The principal object I have in view, is to adduce fome additional proofs, that the bituminous fubftances are derived from the organized kingdoms of nature, and efpecially from vegetable bodies; for, although many circumftances feem to lead to the opinion, that the animal kingdom has in fome meafure contributed to the partial formation of bitumen, yet the proofs are by no means fo numerous, nor fo pofitive, as thofe which indicate the vegetable kingdom to have been the grand fource from which the bitumens have been derived. But this opinion, (founded upon very ftrong prefumptive evidence,) although generally adopted, is however queftioned by fome perfons; and I fhall therefore bring forward a few additional facts, which will, I flatter myfelf, contribute to demonftrate, that bitumen has been, and is actually and immediately formed, from the refin, and perhaps from fome of the other juices of vegetables.

Pure bitumens differ much from vegetable juices;

The chemical characters of the pure or unmixed bitumens, fuch as naptha, petroleum, mineral tar, and afphaltum, are, in certain refpects, fo different from thofe of the refins and other infpiffated juices of recent vegetables, that, had the former never occurred but in a feparate and unmixed ftate, no pofitive inference could have been drawn from their pro-

* Pyrites, ochraceous iron ore, and fahlertz, are alfo occafionally found in the forms of vegetable bodies.

perties

perties in proof of their vegetable origin. Fortunately, however, they have been more frequently found under circumftances which have ftrongly indicated the fource from whence they have been derived; and much information has been acquired from obfervations made on the varieties of turf, bituminous wood, and pit coal, on the nature of their furrounding ftrata, on the veftiges of animal and vegetable bodies which accompany them, and on various other local facts; all of which tend confiderably to elucidate the hiftory of their formation, and to throw light upon this interefting part of geology. but the progreffive changes are inftructive in thefe refpects.

Some inftances have already been mentioned, which fhow that foffil animal fubftances form a feries, commencing with fuch as are fcarcely different from thofe which are recent, and terminating in productions which have totally loft all traces of organization.

Similar inftances are afforded by the vegetable kingdom; but without entering into a minute detail of every gradation, I fhall only cite three examples in this ifland, namely,

1. The fubmarine foreft at Sutton, on the coaft of Lincolnfhire, the timber of which has not fuffered any very apparent change in its vegetable characters *. Series of change. 1. Submarine wood.

2. The ftrata of bituminous wood (called Bovey Coal) found at Bovey, in Devon; which exhibits a feries of gradations, from the moft perfect ligenious texture, to a fubftance nearly approaching the characters of pit coal, and, on that account, diftinguifhed by the name of Stone Coal. 2. Bovey coal in all its gradations.

3. And laftly, the varieties of pit coal, fo abundant in many parts of this country, in which almoft every appearance of vegetable origin has been deftroyed. 3. Perfect pit-coal.

The three examples above-mentioned, appear to form the extremities and centre of the feries; but as, from fome local circumftances, the procefs of carbonization, and formation of bitumen, has not taken place in the firft inftance, and as thefe effects have proceeded to the ultimate degree in the laft, it feems moft proper that we fhould feek for information, and for pofitive evidence, in the fecond example, which appears to be the mean point, exhibiting effects of natural operations, by which bitumen and coal have been imperfectly and partially The fecond ftate offers the moft inftructive fubject for refearch.

* Account of a fubmarine Foreft on the Eaft Coaft of England, by Dr. Correa de Serra. Phil. Tranf. for 1799, p. 145.

formed

formed, without the abfolute obliteration of the original vegetable characters; and, although I have felected the Bovey coal as an example, becaufe it is found in this country, we muft recollect that fimilar fubftances, or ftrata of bituminous wood, are found in many parts of our globe; fo that the example which has been more immediately chofen, is neither rare nor partial *.

The nature, however, of the various kinds of bituminous wood, may in fome refpects be different; but this I have not as yet had the means of afcertaining; I fhall therefore only ftate the facts refulting from experiments made on Bovey coal, and more efpecially on a peculiar bituminous fubftance with which it is accompanied. But, before I enter into thefe particulars, it will be proper to mention a very remarkable fchiftus, with which I was, fome months fince, favoured by the Right Hon. Sir Jofeph Banks.

Remarkable fchiftus,

§ III.

from Iceland,

This fchiftus was found by Sir Jofeph, in the courfe of his tour through Iceland, near Reykum, one of the great fpouting hot fprings, diftant about twenty-four Englifh miles from Hafnifiord; but circumftances did not permit him to afcertain the extent of the ftratum.

confifting very much of alder leaves.

The fingularity of this fubftance is, that a great part of it confifts of leaves, which are evidently thofe of the alder, interpofed between the different lamellæ. I do not mean mere impreffions of leaves, fuch as are frequently found in many of the flates, but the real fubftance, in an apparently half charred ftate, retaining diftinctly the form of the leaves, and the arrangement of the fibres.

External characters.

The fchiftus is light, brittle, of eafy oxfoliation, in the tranfverfe fracture earthy, and of a pale brown colour; but, when longitudinally divided, the whole furface conftantly prefents a feries of the leaves which have been mentioned, uniformly fpread, and commonly of a light gray on the upper furface, and of a dark brown on the other; the fibres on the

* Strata of bituminous wood are found in various parts of France, in the vicinity of Cologne, in Heffe, Bohemia, Saxony, Italy, and efpecially in Iceland, where it is known under the name of Surturbrand.

light gray ſurface being generally of a blackiſh-brown, which is alſo the colour aſſumed by the ſchiſtus when reduced to powder.

The leaves appeared to be in the ſtate of charcoal, by being extremely brittle, by the blackiſh brown colour, by deflagrating with nitre, by the manner of burning, and by forming carbonic acid. I was, however, ſoon convinced that the ſubſtance of theſe leaves was not complete charcoal, but might more properly be regarded as vegetable matter in an incipient ſtate of carbonization, which, although poſſeſſed of many of the apparent porperties of charcoal, ſtill retained a ſmall portion of ſome of the other principles of the original vegetable.

The leaves almoſt in the ſtate of coal;

My ſuſpicion was excited, partly by the odour produced during combuſtion, which rather more reſembled that of wood than that of charcoal, and partly by the brown ſolution formed by digeſting the powder of the unburned ſchiſtus in boiling diſtilled water; for, by various teſts I aſcertained, that the ſubſtance thus diſſolved was not of a mineral nature. In order, however, fully to ſatisfy myſelf in this reſpect, I digeſted 250 grains of the pulverized ſchiſtus with ſix ounces of water.

but burning with the odour of wood.

Digeſtion of the ſchiſtus in water, and examination of the ſolution.

The liquor was, as before, of a dark brown colour.

It had but little flavour.

Pruſſiate of potaſh, muriate of barytes, and ſolution of iſinglaſs, did not produce any effect; nitrate of ſilver formed a very faint cloud; ſulphate of iron was ſlowly precipitated, of a dark browniſh colour; and muriate of tin produced a white precipitate.

A portion of the ſolution, by long expoſure to the air, was partially decompoſed; and a quantity of a brown ſubſtance was depoſited, which could not again be diſſolved in water.

Another portion was alſo evaporated to dryneſs, and afforded a ſimilar brown ſubſtance, which was only partially ſoluble in water; and the reſiduum, in both of the above caſes, was found to be inſoluble in alcohol, and in ether.

When burned, it emitted ſmoke, with the odour of vegetable matter.

250 grains of the ſchiſtus, afforded about three grains of the above ſubſtance; and, when the properties of the aqueous ſolution are conſidered, ſuch as its partial decompoſition, and the depoſit which it yielded by expoſure to air, and by evaporation;

tion; the infolubility of this depofit when again digefted with water, alcohol, or ether; the fmoke and odour which it yielded when burned; and the precipitates formed by the addition of fulphate of iron and muriate of tin to its folution; when thefe properties, I fay, are confidered, there feems much reafon to conclude, that the fubftance diffolved by water was vegetable extract, which had apparently fuffered fome degree of modification, but not fufficient to annul the more prominent characteriftic properties of that fubftance.

The powder of the fchiftus, which had been employed in the preceding experiment, was afterwards digefted in alcohol during two days; and a pale yellow tincture was thus formed, which, by evaporation, left about one grain of a yellow tranfparent fubftance, poffeffing the properties of refin.

It contained vegetable extract, and a little refin,

It appears, therefore, that a fubftance very analogous to vegetable extract, and a fmall portion of refin, remain inherent in the leaves of this remarkable fchiftus.

but no tannin.

As folution of ifinglafs did not produce any effect, there was reafon to conclude, that the aqueous folution above-mentioned did not contain any tannin; but, as the tannin might be combined with the alumina of the fchiftus, I digefted a portion of it in muriatic acid, which, after filtration, was evaporated almoft to drynefs, leaving, however, the acid in a flight excefs. This was diluted with water; and afforded a blue precipitate with pruffiate of potafh, a yellowifh precipitate with ammonia, and a white precipitate with muriate of tin, but not any with folution of ifinglafs. The tannin which might have been contained in the recent vegetable, appears therefore to have been diffipated or decompofed, with the greater part of the other vegetable principles, excepting the woody fibre reduced to the ftate of an imperfect coal, and the fmall portions of extract and refin which have been mentioned.

Previous to having made the analyfis, I had an idea, that this fchiftus might be a lamellated incruftation, formed by the tufa of the hot fprings; but, according to Mr. Klaproth's analyfis *, the tufa of Geyfer is compofed of,

Silica - -	98
Alumina - - -	1.50
Iron - -	50
	100.

* *Beiträge*; *Zweiter band*, p. 109.

It

It is therefore very different from the ſchiſtus, the component ingredients of which were aſcertained from the following analyſis.

ANALYSIS OF THE SCHISTUS FROM ICELAND*.

A. 250 grains, by diſtillation, yielded water, which, in the latter part of the proceſs, became ſlightly acid and turbid, =42.50 grains. Analyſis of the ſchiſtus.

B. The heat was gradually increaſed, until the bulb of the retort was completely red hot. During the increaſe of the heat, a thick brown oily bitumen came over, which weighed 7.50 grains; it was attended with a copious production of hydrogen, carbonated hydrogen, and carbonic acid, the whole of which may be eſtimated at 23.75 grains.

C. The reſiduum was black, like charcoal, and weighed 176.25 grains; but, being expoſed to a ſtrong red heat in a crucible of platina, it burned with a faint lambent flame, and was at length reduced to a pale brown earthy powder, which weighed 122 grains; ſo that 54.25 grains were conſumed.

D. The 122 grains were mixed with 240 of pure potaſh; and, as ſome particles of charcoal remained, 50 grains of nitre were added, and the whole was ſtrongly heated, during half an hour, in a ſilver crucible. The maſs was then diſſolved in diſtilled water, and, muriatic acid being added to exceſs, the liquor was evaporated to dryneſs, and was again digeſted with muriatic acid much diluted; a quantity of pure ſilica then remained, which, after having been expoſed to a red heat, weighed 98 grains.

E. The liquor from which the ſilica had been ſeparated, was evaporated nearly to dryneſs, and added to boiling lixivium of potaſh; after the boiling had been continued for about one hour, the liquor was filtrated, and a quantity of oxide of iron was collected, which amounted to 6 grains.

F. Solution of muriate of ammonia was added to the preceding filtrated liquor; and, the whole being then heated, a copious precipitate of alumina was obtained, which, after having been made red-hot, weighed 15 grains.

Carbonate of ſoda cauſed the preceding liquor (after the ſeparation of alumina) to become ſlightly turbid, but not any precipitate could be collected.

* The remaining ſpecimens are now in the Britiſh Muſeum, and in the collection of the Right Hon. Charles Greville.

By

Component parts.

By this analysis, 250 grains of the schiftus afforded,

			Grains.
Water - - - -	A.	-	42.50
Thick brown oily bitumen	B.	-	7.50
Mixed gas (by computation)	B.	-	23.75
Charcoal (by computation)	C.	-	54.25
Silica - - - - -	D.	-	98
Oxide of iron - -	E.	-	6
Alumina - - - - -	F.	-	15
			247.

But the water and vegetable matter muſt be regarded as extraneous; and, if they are deducted, the real compoſition of the ſchiſtus is nearly as follows.

Its component parts, excluſive of water and vegetable matter.

Silica - - - - -	82.30
Alumina - - - - -	12.61
Oxide of Iron - - - -	5.
	99.91.

It evidently, therefore, belongs to the family of argillaceous ſchiſtus, although the proportion of ſilica is more conſiderable than has been found in thoſe hitherto ſubjected to chemical analyſis.

The ſchiſtus has not been noticed by von Troil, nor by any of thoſe who have written concerning Iceland; for the ſlate which was ſent to Profeſſor Bergmann by the former, and which is mentioned by the latter in one of his letters, is there expreſsly ſtated to be the common aluminous ſlate containing impreſſions *.

§ IV.

Queſtion, whether the circumſtances of the production of this ſchiſtus were peculiar.

From the experiments which have been related, we find that the leaves contained in the Iceland ſchiſtus, although they are

* Letters on Iceland, by Uno von Troil, p. 355.

Mr. Faujas St. Fond has however deſcribed a ſchiſtus nearly ſimilar, which is found near Roche-Seauve, in the Vivarais. The ſtratum extends about two leagues; and the only difference is, that, according to Mr. St. Fond, the ſchiſtus at Roche-Seauve is of the nature of marble, or, as he terms it, argillo-calcareous, whereas this of Iceland is ubdoubtedly argillaceous. From Mr. St. Fond's account, it does not appear that the vegetable leaves contained in the ſchiſtus of Roche-Seauve have been chemically examined. *Eſſai de Geologie*, par. B. Faujas St. Fond, Tome I. pp. 128 and 134.

apparently

apparently reduced almoft to the ftate of charcoal, neverthelefs retain fome part of their original proximate principles, namely, extract and refin. This, of itfelf, is undoubtedly a remarkable fact; but, if it were unfupported by any other, the only inference would be, that the fchiftus was moft probably of very recent formation, and had been produced under peculiar circumftances.

I was defirous, therefore, to difcover fome fimilar cafes, which might ferve as additional corroborative proofs of the gradual alterations by which vegetable bodies become changed, fo as at length to be regarded as forming part of the mineral kingdom; and, from the reafons which have been ftated in the commencement of this paper, as well as from a certain fimilarity in the external characters of the fubftance compofing the leaves above-mentioned with thofe of the Bovey coal, I was induced to make this laft alfo a fubject of chemical inquiry.

requires an attention to other mineral bitumens.

In the Philofophical Tranfactions for the year 1760*, fome remarks on the Bovey coal, and an account of the ftrata, are ftated, in a letter from the Rev. Dr. Milles to the Earl of Macclesfield. The object, indeed, of the author, was to eftablifh that this and fimilar fubftances are not of vegetable, but of mineral origin; and, to prove this, he adduces a great number of cafes, moft of which, however, in the prefent ftate of natural hiftory and of chemiftry, muft be regarded as proving the contrary; whilft others, mentioned by him, fuch as the Kimmeridge or Kimendge coal, are nothing more than bituminous flates, and of courfe are of a very different nature.

Bovey coal.

Dr. Milles's account of the varieties of the Bovey coal, and of the ftate of the pits at that time, appears to be very accurate; and, for the prefent ftate, or at leaft fuch as it was in 1796, I fhall beg leave to refer to a paper of mine, publifhed in the fourth volume of the Tranfactions of the Linnean Society †; for, as this is more immediately a chemical inveftigation, I wifh to avoid, as much as poffible, entering into any minute detail of geological circumftances.

Its hiftory.

* Vol. LI. p. 534.

† Obfervations on bituminous Subftances, p. 138. See alfo Parkinfon's Organic Remains of a former World. Vol. I. p. 126.

It

Its ſtrata agree with thoſe of ſurturbrand.

It may however be proper to obſerve, that the Bovey coal is found in ſtrata, correſponding in almoſt every particular with thoſe of the ſurturbrand in Iceland, deſeribed by Von Troil *, and by Profeſſor Bergmann †. The different ſtrata of both theſe ſubſtances are likewiſe ſimilar, being compoſed of wood or trunks of trees, which have completely loſt their cylindrical form, and are perfectly flattened, as if they had been ſubjected to an immenſe degree of preſſure ‡.

The

* Von Troil's Letters, p. 42.

† *Opuſcula Bergmanni*, Tom. III. *De Productis Volcaniis*, p. 239.

‡ Bergmann, in the diſſertation above quoted, accurately deſcribes this appearance of the ſurturbrand, and then ſays, "Quæ autem immanis requiritur vis, ut truncus cylindricus ita complanetur? *Nonne antea particularum nexus putredinis quodam gradu fuerit relaxatus?* Certe, niſi compages quodammodo mutatur, quodlibet pondus incumbens huic effectui erit impar. Ceterùm idem obſervatur phænomenon *in omni ſchiſto argillaceo.*" This is certainly a very curious fact; and the learned Profeſſor, with his uſual acuteneſs, rejects the idea that mere weight can have been the cauſe. As a farther proof alſo, he afterwards obſerves, "Orthoceratitæ, quæ *in ſtrato calcareo conicam figuram perfectè ſervant*, in ſchiſto *planum fere triangulare compreſſione efficiunt.* Idem valet de piſcibus, conchis, inſectiſque petrefactis." And again, "Obſervatu quoque dignum eſt, quod *idem reperiatur effectus, quamvis ſtratum calcareum ſub ſchiſto collocatum ſit, et majori ideo pondere comprimente onuſtum.*" *De Productis Volcaniis*, p. 240. It is evident, therefore, that weight alone has not produced this effect; and Bergmann's idea, that the ſolidity of the vegetable bodies may have undergone ſome previous change, in the manner of incipient putrefaction, by moiſture, and by becoming heated in the maſs, muſt be allowed to be very probable. But bodies ſuch as ſhells could not be thus affected; and therefore they muſt have been expoſed to ſome mechanical effect, peculiar to argillaceous ſtrata; which effect, however, from the circumſtances which have been adduced, evidently could not have reſulted from the mere preſſure of the ſuperincumbent ſtrata. To me, therefore, it ſeems not very improbable, that, together with a certain change in the ſolidity of vegetable bodies, produced in the manner imagined by Bergmann, and, together with ſome degree of ſuperincumbent preſſure, a real and powerful mechanical action has been exerted, by the contraction of the argillaceous ſtrata, in conſequence

of

The Bovey coal is commonly of a chocolate-brown, and ſometimes almoſt black. The quality and texture of it are various in different ſtrata; from ſome of theſe, it is obtained in the form of ſtraight flat-pieces, three or four feet in length, reſembling boards, and is therefore called Board Coal. Others have an oblique, wavy, and undulating texture, and, as Dr. Milles obſerves, have a ſtrong reſemblance to the roots of trees, from which, moſt probably, this ſort has in a great meaſure been formed.

External characters of Bovey coal.

Some kinds alſo appear to be more or leſs intermixed with earth; but that which produces the moſt powerful and laſting fire, is called ſtone coal; it is black, with a gloſſy fracture; has little or none of the vegetable texture; is more ſolid and compact than the others, being almoſt as heavy as ſome of the pit coals, the nature of which it ſeems very nearly to approach.

For chemical examination, I ſelected ſome of the coal which had a wavy texture, and rather a gloſſy fracture; the quality of this ſort being apparently intermediate between the others, as it retains completely the marks of its vegetable origin, while, at the ſame time, it poſſeſſes every perfect character of this ſpecies of coal.

A. 200 grains of the Bovey coal, by diſtillation, yielded,

Chemical examination of Bovey coal.

		Grains.
1. Water, which ſoon came over acid, and afterwards turbid, by the mixture of ſome bitumen - -		60
2. Thick brown oily bitumen - - - -		21
3. Charcoal - - - - - - - -		90
4. Mixed gas, conſiſting of hydrogen, carbonated hydrogen, and carbonic acid,	eſtimated at	29
		200.

The charcoal, in appearance, perfectly reſembled that which is made from recent vegetables. By incineration, about 4 grains of yellowiſh aſhes were left, which conſiſted of alumina, iron, and ſilica, derived moſt probably from ſome ſmall portion of the clay ſtrata which accompany the Bovey coal.

of deſiccation; this, I believe, has not hitherto been much conſidered, but I am inclined, from many circumſtances, to attribute to it a very great degree of power.

But

But it is very remarkable, that neither the afhes obtained from the charcoal of the Bovey coal, nor thofe obtained from the leaves of the Iceland fchiftus, afforded the fmalleft trace of alkali.*

B. 200 grains of the Bovey coal, reduced to powder, were digefted in boiling diftilled water, which was afterwards filtrated, and examined; but I could not difcover any figns of extract, or of any other fubftance.

C. 200 grains were next digefted with fix ounces of alcohol, in a very low degree of heat, during five days. A yellowifh-brown tincture was thus formed, which, by evaporation, afforded a deep brown fubftance, poffeffing all the properties of refin, being infoluble in water, but foluble in alcohol, and in ether; it alfo fpeedily melted, when placed on a red-hot iron, burned with much flame, and emitted a fragant odour, totally unlike the very unpleafant fmell produced by burning the coal itfelf, or by burning any of the common bituminous fubftances. The quantity, however, which could be extracted from 200 grains of the coal, by alcohol, was but fmall, as it did not exceed 3 grains. But this fmall quantity was fufficient to prove, that although the Bovey coal does not contain any vegetable extract, like the fchiftus formerly mentioned, yet the whole of the proximate principles of the original vegetable have not been entirely changed; as a fmall portion of true refin, not converted into bitumen, ftill remains inherent in the coal, although the bituminous part is much the moft prevalent, and caufes the fetid odour which attends the combuftion of this fubftance.

It refembles the fchiftus in its products as well as character; but it afforded no extract, but only refin;

Upon a comparifon of the general external characters of the Bovey coal, with thofe of the fubftance which forms the leaves contained in the Iceland fchiftus, a very great refemblance will be obferved; and this is farther confirmed, by the fimilarity of the products obtained from each of them in the preceding experiments, with the fingle exception, that the leaves contain

* This, as far as relates to the Bovey coal, has been alfo noticed by Dr. Milles, Phil. Tranf. Vol. LI. p. 553. But wood, however long fubmerged, is not deprived of alkali, unlefs it has more or lefs been converted into coal; for I have, fince the reading of this paper, made fome experiments on the wood of the fubmerged foreft at Sutton, on the coaft of Lincolnfhire, and have found it to contain potafh.

fome

ſome vegetable extract, which I could not diſcover in the Bovey coal. They agree however in every other reſpect; as they both conſiſt of woody fibre in a ſtate of ſemicarbonization, impregnated with bitumen, and a ſmall portion of reſin, perfectly ſimilar to that which is contained in many recent vegetable bodies; and thus it ſeems, that as the woody fibre, in theſe caſes, ſtill retains ſome part of its vegetable characters, and is but partially and imperfectly converted into coal, ſo, in like manner, ſome of the other vegetable principles have only ſuffered a partial change. Undoubtedly, there is every reaſon to believe that, next to the woody fibre, reſin is the ſubſtance which in vegetables paſſing to the foſſil ſtate, moſt powerfully reſiſts any alteration; and that, when this is at length effected, it is more immediately the ſubſtance from which bitumen is produced. The inſtances which have been mentioned corroborate this opinion; for the vegetable extract in one of them, and more eſpecially the reſin which was diſcovered in both, muſt be regarded as part of thoſe principles of the original vegetables which have remained, after ſome other portions of the ſame have been modified into bitumen.

which moſt probably is that which affords the bitumen.

The ſmallneſs of the quantity of reſin obtained in both the preceding caſes, by no means invalidates the proof of the above opinion; but, as an additional confirmation of it, I ſhall now give an account of a very ſingular ſubſtance, which is found with the Bovey coal.

§ V.

Singular ſubſtance found with the Bovey coal.

Dr. Milles, in his remarks on the Bovey coal, (which I have ſeveral times had occaſion to notice in the courſe of this Paper,) ſtates, that "amongſt the clay, but adhering to the coal, are found lumps of a bright yellow *loam*, extremely light, and ſo ſaturated with petroleum, that they burn like ſealing wax, emitting a very agreeable and aromatic ſcent."*

It is a peculiar bitumen.

This ſubſtance, I alſo obſerved, when I viſited the Bovey coal-pits, in 1794 and 1795. At that time, however, it was ſcarce, and I could only procure one ſmall ſpecimen, which is now in the Britiſh Muſeum; but, from a curſory examination of it, I was convinced that it was a peculiar bituminous ſubſtance,

* Phil. Tranſ. Vol. LI. p. 536.

and not loam impregnated with petroleum, as Dr. Milles had fuppofed. I could not then conveniently make a regular analyfis of it, and therefore contented myfelf with briefly defcribing it, in a note annexed to my Paper on bituminous Subftances. *

Lately, however, my friend John Sheldon, Efq. of Exeter, F. R. S. obligingly fent me feveral pieces of it, together with fpecimens of the different kinds of Bovey coal which have been mentioned; and thus I was enabled fully to afcertain its real nature and properties.

DESCRIPTION OF THE BITUMEN FROM BOVEY.

Defcription of the bitumen from Bovey.

It accompanies the Bovey coal, in the manner already defcribed, and is found in maffes of a moderate fize.

The colour is pale brownifh ochraceous yellow.

The fracture is imperfectly conchoidal.

It appears earthly externally, but, when broken, exhibits a flight degree of vitreous luftre.

The fragments are irregularly angular, and completely opaque at the edges.

It is extremely brittle.

It does not apparently become foftened, when held for fome time in the hand, but emits a faint refinous odour.

The fpecific gravity, at temperature 65° of Fahrenheit, is, 1,135.

Some fpecimens have dark fpots, flightly approaching in colour and luftre to afphaltum; and fmall portions of the Bovey coal are commonly interfperfed in the larger maffes of this bitumen.

When placed on a heated iron, it immediately melts, fmokes much, burns with a bright flame, and yields a very fragrant odour, like fome of the fweet-fcented refins, but which at laft becomes flightly tainted with that of afphaltum.

The melted mafs, when cold, is black, very brittle, and breaks with a gloffy fracture.

EXPERIMENTS.

Experiments on the fame.

A. 100 grains of this bitumen, when diftilled until the bulb of the retort became red-hot, afforded,

* Tranfactions of the Linnean Society, Vol. IV. p. 139.

1. Water

	Grains.
1. Water flightly acid - - - - - -	3
2. Thick brown oily bitumen, very fimilar to that which was obtained from the Bovey coal, but poffeffing flightly the odour of vegetable tar - - -	45
3. Light fpongy coal - - - - - -	23
4. Mixed gas, compofed of hydrogen, carbonated hydrogen, and carbonic acid, (by computation,) -	29.

The coal yielded about three grains and a half of afhes, which confifted of alumina, iron, and filica, with a trace of lime.

B. The bitumen was not affected by being long digefted in boiling diftilled water.

C. By digefting 100 grains in lixivium of pure potafh, a brown folution was formed; this was faturated with muriatic acid, and a brown refinous precipitate was obtained, which weighed 21 grains.

D. A portion was digefted in nitric acid: at firft, much nitrous gas was evolved, and, after the digeftion had been continued for nearly 48 hours, a part was diffolved, and formed an orange-coloured folution, which did not yield any precipitate, when faturated by the alkalis, or by lime; the colour only became more deep, and, by evaporation, a yellow vifcid fubftance was obtained, which was foluble in water. The above nitric folution poffeffed every property of thofe nitric folutions of refinous fubftances which I have mentioned in a former paper. *

E. The benzoic and fuccinic acids were not obtained from this fubftance, by any of the methods ufually employed.

F. Alcohol almoft immediately began to act upon this bitumen; and, being added at different times, gradually diffolved a confiderable part of it. The folution was reddifh-brown, and has a refinous odour; by the addition of water it became milky, and, by evaporation, afforded a dark brown fubftance, which had every property of refin, whilft the refiduum left by the alcohol poffeffed thofe proporties which characterize afphaltum.

The following analyfis was then made, to difcover the proportions of the component ingredients.

* Phil. Tranf. for 1804, p. 198.

ANALYSIS OF THE BITUMEN FROM BOVEY.

Analyfis of the bitumen from Bovey.

A. 100 grains, reduced to a fine powder, were digefted during 48 hours with fix ounces of alcohol, the veffel being placed in fand moderately warmed. A deep reddifh-brown tincture was thus obtained; and the operation was again twice repeated, with other portions of the fame menftruum, until it ceafed to act upon the refiduum.

The whole of the fpirituous folution (which had been cautioufly decanted) was then fubjected to a very gradual diftillation in an alembic, and yielded a brown fragant refin, which weighed 55 grains.

B. The refiduum, which could not be diffolved by alcohol, was digefted in boiling diftilled water, but this did not act upon it; the whole was therefore collected on a filter, was gradually dried, without heat, by mere expofure to the air, and then weighed 44 grains.

Thefe 44 grains confifted of a light, porous, pale-brown fubftance, which, being melted, formed a black, fhining, brittle mafs. It burned with the odour of afphaltum, but rather lefs difagreeable, owing moft probably to a fmall portion of the refin, which had not been completely extracted by the alcohol. It was infoluble in water, and in alcohol, but was readily diffolved by heated fat oils; and in every other particular was found to poffefs the properties of afphaltum.

The 44 grains of afphaltum, when burned, left a refiduum, which weighed three grains, and confifted of alumina, filica, and iron.

Its component parts.

By this analyfis it appears, that the bitumen which accompanies the Bovey coal, is a peculiar and hitherto unknown fubftance, which is partly in the ftate of vegetable refin, and partly in that of the bitumen called afphaltum, the refin being in the largeft proportion, as 100 grains of the above-mentioned fubftance afforded,

Refin - - - - - -	55
Afphaltum - - - - -	41
Earthy refiduum - - -	3
	99.

Thus we have an inftance of a fubftance being found under circumftances which conftitute a foffil, although the characters of it appertain partly to the vegetable, and partly to the mineral kingdom.

§ VI.

§ VI.

The powerful action which alcohol exerts on most of the resins, may justly be regarded as forming a marked distinction between those substances and the bitumens. But, as some of the bitumens are acted upon by alcohol, in a slight degree, I was desirous to ascertain whether a small portion of resin was contained in any of these; or, if that was not the case, I wished to determine the nature of the substances which could be separated, although very sparingly, by this menstruum. I therefore made the following comparative experiments, on the soft brown elastic bitumen from Derbyshire; on the genuine asphaltum; on very pure cannel coal; and on the common pit coal.

Experiments on bitumens by the action of alcohol.

100 grains of each were digested with three ounces of alcohol, in matrasses placed in warm sand, during five days, some alcohol being occasionally added, to supply the loss caused by evaporation. After the above-mentioned period had elapsed, the liquid contained in each matrass was poured into separate vessels.

Results.

I. The alcohol which had been digested on the elastic bitumen was not tinged, nor, when spontaneously evaporated, did it leave any film or stain on the glass.

II. From asphaltum, the alcohol had extracted a yellow tincture, which, in some situations, appeared of a pale olive colour, and, being spontaneously evaporated, a thick brown liquid was deposited, in small drops, on the glass; these drops did not become hard after two months, and possessed the odour, and every other property, of petroleum. The asphaltum had lost in weight about one grain and a half.

III. The cannel coal had communicated a pale yellow tint to the alcohol, which, in the manner above described, was ascertained to be caused by petroleum; but, from the smallness of the quantity, the weight could not be determined.

IV. The alcohol which had been digested on pit coal, had not assumed any colour; but, by spontaneous evaporation *, it left a film on the glass, which, by its odour, was also found to be petroleum.

Alcohol acts slightly on bitumens.

By these experiments we find, that the action of alcohol on the bitumens is very slight; and that the small portion which may

* Spontaneous evaporation, by exposure to the air, was employed in these experiments, for reasons which must be sufficiently obvious.

may thus be extracted from some of them, is petroleum. In these, the process of bituminization (if I may be allowed to employ such a term) appears to have been completed, whilst in the Bovey coal, and especially in the substance which accompanies it, nature seems to have performed only the half of her work, and, from some unknown cause, to have stopped in the middle of her operations. But, by this circumstance, much light is thrown on the history of bituminous substances; and the opinion, that they owe their origin to the organized kingdoms of nature, especially to that of vegetables, which hitherto has been supported only by presumptive proofs, seems now, in a great measure, to be confirmed, although the causes which operate these changes on vegetable bodies are as yet undiscovered.

They are not formed by time alone.

Many facts indicate, that time alone does not reduce animal or vegetable bodies to the state of fossils. In this country, there are numerous examples of large quantities of timber, (even whole forests) which have been submerged prior to any tradition, and which nevertheless completely retain their ligneous characters.* Other local causes and agents must therefore have been required, to form the varieties of coal and other bituminous substances. In some instances (as in the formation of Bovey coal), these causes seem to have acted partially and imperfectly, whilst, in the formation of the greater part of the pit coals, their operation has been extensive and complete.

In the pit coals, the mineral characters predominate, and the principal vestige of their real origin seems to be bitumen; for the presence of carbon in the state of oxide, cannot alone be considered as decisive.

Bitumen, therefore, with the exuviæ and impressions so commonly found in the accompanying strata, must be more immediately regarded as the proofs, in favour of the origin of pit coal from organized bodies; and, considering the general facts which have been long observed, together with those lately adduced respecting the Bovey coal, and the substance which is found with it, we seem now to have almost unques-

* Phil. Transf. for January, 1671. Phil. Transf. Vol. XIX. p. 526. Ibid. Vol. XXII. p. 980. Ibid. Vol. XXIII. p. 1073. Ibid. Vol. XXVII. p. 298. Ibid. for 1799, p. 145.

tionable

tionable evidence, that bitumen has eſſentially been produced by the modification of ſome of the proximate principles of vegetables, and eſpecially reſin.

Modern chemiſtry had comparatively made but a ſmall progreſs, when the illuſtrious Bergmann publiſhed his Diſſertation, entitled, *Producta Ignis ſubterranei chemice conſiderata*; for, at that time, the extent and power of chemical action, in the humid way, were very imperfectly underſtood. In that part, however, of the above work, where he ſpeaks of the foſſil wood of Iceland, called Surturbrand, he evidently appears doubtful how far volcanic fire may have acted upon it; although he conceives that, in the formation of it there has been ſome connection with volcanic operations. His words are, "*Quid de ligno foſſili Iſlandiæ ſentiendum ſit, gnaro in loco natali contemplatori* decidendum relinquimus. *Interea, ut cum vulcani operationibus nexum credamus, plures ſuadent rationes, quamvis hucuſque modum ignoremus, quo ſitum texturamque adquiſiverunt hæc ſtrata.*" It certainly was very natural that Bergmann ſhould entertain this opinion, in reſpect to the ſurturbrand; and it is remarkable, that the leaves contained in the ſchiſtus lately deſcribed, are of the ſame nature, and are found in the ſame country. The leaves alſo deſcribed by Mr. St. Fond, are likewiſe found in a country which, according to him, was formerly volcanic. Were theſe ſubſtances, therefore, never found but in countries which either actually are or were volcanic, we ſhould be almoſt compelled to believe, with the Swediſh profeſſor, that the operations of ſubterraneous fires have been concerned in the formation of theſe bodies, or rather in the converſion of them into their preſent ſtate.

Whether they be of igneous or aqueous formation.

But ſimilar ſubſtances are found in countries where not the ſmalleſt veſtige of volcanic effects can be diſcovered, and Devonſhire moſt undoubtedly is ſuch; yet, nevertheleſs, the Bovey coal is there found ſimilar to the ſurturbrand, in moſt of the external, and (from experiments which I made ſome years ago, I believe I may ſay) chemical properties; to which muſt be added, that both theſe ſubſtances perfectly reſemble each other, by forming regular ſtrata.*

They are found in countries evidently not volcanic.

* Tranſ. of the Linnean Society, Vol. IV. p. 138. Von Troil's Letters, p. 42. *Opuſcula Bergmanni*, Tom. III. p. 239.

Moreover,

Deduction.

Moreover, the half charred appearance of Bovey coal, and of furturbrand, cannot be adduced as any proof, that the original vegetable bodies have been expofed to the partial effects of fubterraneous fire; for, at this time, we know that the oxidizement of fubftances is performed, at leaft as frequently, and as effectually, by the humid as by the dry way. It would therefore be fuperfluous here, to enter into an elaborate difcuffion, to prove that coal and bitumen, with much greater probability, have been formed without the intervention of fire; and I am the lefs inclined to fay more upon this fubject, as I have already publifhed fome confiderations on it in a former paper.*

The new fubftance found with the Bovey coal termed *Retinafphaltum.*

Before I conclude, I muft beg leave to obferve, that as the fubftance which is found with the Bovey coal is, in every refpect, fo totally different from any of the bitumens hitherto difcovered, it feems proper that it fhould receive fome fpecific name; and, as it has been proved to confift partly of a refin and partly of a bituminous fubftance, I am induced to call it *Retinafphaltum,*† a name by which a full definition of its nature is conveyed.

Account of another fpecimen, probably of the fame.

I have lately feen, in No. 85 of the *Journal des Mines*, p. 77, an account of a peculiar combuftible foffil, found near Helbra, in the county of Mansfield, and defcribed by Mr. Voight, in his *Verfuch einer Gefchichte der Steinkohle, der Brawnkohle*, &c, p. 188. This fubftance is of an afh-coloured gray, paffing to grayifh-white; it is found in a bed of bituminous vegetable earth, which has apparently been produced by the decompofition of foffil wood. The pureft fpecimens are in the form of nodules; the fracture is earthy; it is opaque; foft; brittle; and is very light. When applied to the flame of a candle, it burns and melts like fealing-wax, at the fame time diffufing an odour which is not difagreeable. This fubftance appears to accord in fo many properties with the retinafphaltum of Bovey, that I cannot but fufpect it to be of a fimilar nature, and I have little doubt that, by a chemical examination, it will be found to confift partly of refin and partly of bitumen.

* Tranf. of the Linnean Society, Vol. IV. pp. 141, &c,

† From ῥητίνη, refin; and ἄσφαλτος, bitumen.

Account

VIII.

Account and Defcription of an improved Air Pump. By Mr. N. MENDELSSOHN.

To Mr. NICHOLSON.

SIR,

ON entering into bufinefs, as a mathematical inftrument-maker, I refolved to make it my principal ftudy to introduce into them all the improvements which the prefent advanced ftate of fcience, the nature of the fubject, and my humble abilities would allow. Thus I have begun with improving *Volta's electrical lamp* and the *air-pump*. I imagine, I have rendered the conftruction of the latter much more fimple than it ufually is, and confequently lefs liable to derangement, at the fame time that it poffeffes a very great exhaufting power. Give me leave, Sir, to offer you a defcription of this improved air-pump with the requifite drawings. It is ready for your infpection. I fhall think myfelf very happy, if this ufeful philofophical inftrument meet with your approbation. And if it be fo fortunate, I requeft you the favour of introducing it into public notice, by inferting my paper in your excellent and defervedly celebrated Journal.

Introductory letter.

I am, with the higheft efteem, Sir,

Your humble and obedient fervant,

N. MENDELSSOHN.

No. 50, *Surry-Street, Black-Friars,*
Feb. 13, 1805.

Notwithftanding the many improvements which have been made upon the conftruction of the air-pump, it was ftill defirable that this inftrument fhould be more fimplified in its mechanifm, and yet poffefs the fame advantages of thofe conftructed lately. That the mechanical power of the pump, and not the preffure of the air, fhould open the valves, has long been required and already done by Mr. Cuthbertfon, Mr. Haas, and feveral other fkilful artifts; but may I be allowed to remark, that on reading the defcription of their inftruments, they appeared to me to be too compounded. It muft be very difficult for

Defcription of Mendelffohn's improved air-pump.

Defcription of Mendelffohn's improved air-pump.

for an experimental philofopher to clean an inftrument which, being thus complicated, is not only rendered intricate, but is alfo difficult to be put together again. Being defirous, therefore, to fimplify this inftrument, I adopted the conftruction here defcribed, by which it is capable of being put together in lefs than half an hour when cleaned, and requires that operation very feldom.

I fubmit to the judgment of your fcientific readers how far the prefent inftrument anfwers its defired purpofe. I have rejected that tube which, in common air-pumps, leads from the valves to the receiver, together with the cock that ferves to fhut this pipe: the receiver is placed immediately upon the valves, thefe being put on the top of the cylinders, which, confequently, required the rackwork and pinion to be underneath, and inverted the whole inftrument. *See the adjoining drawing, Plate* VI. where A B and C D reprefent the two cylinders of glafs ground and polifhed infide. E and F are the two valves that allow the cylinders to communicate with the receiver O through two very fhort canals A B and C D (*Fig* 2, *Plate* VII) and the cock G. Two other valves that open into the atmofphere are within the covers *i* and *k*, as may be feen in *Fig*. 1, where *e* reprefents one of them. M N is the receiver-plate of glafs ground flat; P Q a barometer-gauge, upon the plan of the firft Torricellian tube, as the eafieft to conftruct and the moft infallible in its effects. It will be found to be here quite out of the way, fecure from being broke by accident, and the moft in fight. H K and I L are two brafs pillars that fupport the whole. R S V W the ufual rackwork, having a double winch *l m*, which, upon trial, will be found preferable to a fingle one.

It will now be neceffary to fhew how this pump acts, in which it will be fufficient to explain the action of one cylinder, becaufe the other is in all parts alike. E is a conical metallic valve, from which a canal goes through the cock G up to the receiver, as is feen in *Fig*. 1 and 2, *Plate* VII. where all the parts are marked with the fame letters. E T is a fteel rod going through a leather box in the pifton U. The top of this rod is fixed to the valve E, and its bottom part flides in a fmall hole with an allowance of 0,1 inch up and downward, confequently the valve E can move no farther. When the pifton defcends, it firft opens the valve by pufhing the rod to the

the bottom of the hole. Then it flides down along the rod E T, and the air from the receiver has now free accefs to the cylinder. When the pifton returns it lifts the rod E T, and thus fhuts up the valve. Then the pifton flides again along the rod up to the top of the cylinder, condenfing the air above it, which air, by the leaft condenfation, opens a valve *e*, *Fig.* 2, and efcapes freely into the atmofphere. This laft valve has neither fpring or additional weight to fhut it, but fhuts by its own weight (about a quarter of an ounce) as foon as the pifton is arrived to the top of the cylinder. Defcriptiou of Mendelffohn's improved air-pump.

The cylinders are made of glafs, *and the piftons of tin, fo well fitted as to be air-tight, without the interpofition of any leathers*. The friction of thefe two bodies is fmall beyond expectation, a fufficient proof that they will be durable. They poffefs the further advantage of being capable of ftanding for even fix months, after which time they will ferve without being cleaned or repaired, becaufe they are not liable to be corroded by the oil which they contain, an inconvenience too general in brafs cylinders. After all, if the prefent pump fhould want cleaning, it is an eafy operation to take off the top piece *g h*, by unfcrewing the nuts H and I, when this piece, with all the apparatus upon it, will come off. Then each cylinder may very eafily be flid off from the pifton, wiped out and replaced, after having greafed its infide with a little of the cleaneft fweet oil: The top is then to be put again in its place, and the two nuts H and I being fcrewed upon it, the inftrument is ready. Neither racks or pinion need to be taken out of their places, the cylinders ftanding above them.

The cock is conftructed fo, that, being in the fituation reprefented in *Fig.* 1, the communication is open between the cylinders, the receiver, and the barometer-gauge, and, by a quarter of a revolution, the cylinders are excluded, the receiver and gauge being ftill left in communication. A little ftopper in *Fig.* 3, ground into the cock, being open, air is admitted to the receiver if required.

The receiver-plate is of glafs ground flat, as was mentioned before: this will be found preferable to brafs, becaufe cleaner, and never corroded by acids or water; it will befides often prove very convenient in making experiments on electricity in the vacuum.

The

The whole inſtrument is fixed upon a mahogany table, which ſerves as a ſtand to it.

I will conclude by obſerving, that neither the employing of glaſs cylinders, or the method of opening the valves, are new, but, for aught I know, this is the firſt inſtrument of the kind ever executed in this country; as likewiſe the idea of putting the valves at top, and thus ſimplifying the inſtrument, ſeems to have eſcaped the attention of the eminent artiſts both here and abroad; as to my beſt knowledge, it has never been done or deſcribed any where. The metallic piſtons, without leathering, muſt certainly add to the durability, and diminiſh the great labour that uſually attends working an air-pump.

IX.

Letter concerning Palladium, from WILLIAM HYDE WOLLASTON, *M. D. F. R. S. the Diſcoverer of that Metallic Body.*

To Mr. NICHOLSON.

SIR,

The author adverts to former proceedings reſpecting palladium; of which he is the diſcoverer.

THE candour with which you communicated all circumſtances that came to your knowledge concerning palladium, at a time when the diſcoverer of that ſubſtance was yet unknown to you, demands my earlieſt acknowledgments, as having been the author of thoſe communications; and it is proper that I ſhould alſo expreſs the ſatisfaction I received on learning the reſpectable tribunal you nominated at my requeſt, for examining the merits of any attempts that might be made to form that ſubſtance artificially.

Reaſons why the diſcovery was firſt communicated to the world anonymouſly.

As I have already ſhewn (in a paper which you did me the honour to reprint in your Journal for January laſt, p. 34,) by what means a very ſmall quantity of Palladium may be extracted from the ore of platina, and as I have there examined the ſynthetic attempts to prove that this body was a compound, with a degree of attention which I thought due to the chemical ſkill of the perſon who propoſed them, as well as to the degree of uncertainty that muſt attend a ſubject entirely new; I cannot now adduce further *chemical* evidence, and can only add, for the information of thoſe whoſe judgment has been biaſſed by the difficulty of accounting for the production

duction of so large a quantity of palladium as was offered for sale, that a proportional quantity of platina, from which the whole was extracted, was purchased by me a few years since, with the design of rendering it malleable for the different purposes to which it is adapted. That object has now been attained, and during the solution of it, various unforeseen appearances occurred, some of which led me to the discovery of palladium; but there were other circumstances which could not be accounted for by the existence of that metal alone. On this, and other accounts, I endeavoured to reserve to myself a deliberate examination of those difficulties which the subsequent discovery of a second new metal, that I have called rhodium, has since enabled me to explain, without being anticipated even by those foreign chemists, whose attention has been particularly directed to this pursuit.

I remain, Sir,

Your obliged and obedient Servant,

W. H. WOLLASTON.

Feb. 23, 1805.

X.

Short Remark on Mr. Walker's last Letter respecting Focal Images. By C. L.

To Mr. NICHOLSON.

SIR,

C. L. declines any farther discussion respecting Mr. Walker's experiments with lenses; but denies the facts.

I WOULD beg your indulgence for a very short letter, in answer to Mr. Ezekiel Walker. The only reply which seems needful, on the subject of the temper in which he or I may have written, is that if his first paper had indicated more of the calm spirit of philosophy, as well as of philosophical correctness, my observations might either have been unnecessary, or, perhaps, drawn up without any extraneous remark. As it is, I do not think myself entitled, nor am I indeed inclined to offer any strictures upon his last; which I leave to the unbiassed judgment of your readers. On the present occasion, I only wish to adhere to the experiments. Men of science will find no difficulty in forming a proper estimate of our reasonings. In plain language, therefore, I will beg leave

to

to wave any attention to the hiſtory of Mr. Walker's experiments, upon which I have animadverted, and to deny his facts. What may have deceived him in his proceedings, his admeaſurements, and his repeated regiſterings, is not for me to diſcuſs. If he will ſend his lenſes to you, and you ſhould find that, under any circumſtances whatever of diſtance or poſition, it be poſſible, by a mere alteration in the aperture, to produce a difference, as 2 to 3, in the length of the focal image, (ſee Philoſ. Journal, Vol. IX. p. 165.) I think it will be incumbent upon us to re-examine all the facts and demonſtrations, of what we have hitherto been in the habit of calling the ſcience of optics, in order to reconcile them with ſo ſtrange a reſult.

I am, Sir,
Your obliged
C. L.

XI.

A Communication on the Uſe of Green Vitriol, or Sulphate of Iron, as a Manure; and on the Efficacy of paring and burning depending, partly, on Oxide of Iron. By GEORGE PEARSON, M. D. *Honorary Member of the Board of Agriculture, F. R S. From a Communication made by him to the Board, and inſerted in the fourth Volume of their Tranſactions.*

Sulphate of iron or martial vitriol hitherto ſuppoſed deſtructive of vegetation,

I TAKE leave to lay before this Honourable Board, an account of a ſubſtance as a manure, which I find, on examination, is one of the things, hitherto univerſally believed to be a poiſon to vegetables. Having aſcertained that this ſubſtance is what is commonly known by the name of vitriol of iron (the ſulphate of iron of the chemiſts), inveterate opinion prevented me for ſome time from accepting the teſtimony of it as a manure; but feeling the weight of the reſpectable evidence by whom it was atteſted, after conſideration I perceived that the fact in queſtion was not at variance with eſtabliſhed principles of vegetable philoſophy, as I ſhall, I think, make appear in this communication.

—but the contrary is true.

My friend John Williams Willaume, Eſq. of Tingrith in Bedfordſhire, having deſired his brother, Charles Dymoke Willaume, Eſq. to aſk my opinion of a ſaline ſubſtance collected

lected from peat, which has been ufed with profitable confequences as a manure in his neighbourhood; I propofed a fet of queries to Mr. John W. Willaume, the anfwers to which, in the two following copied letters, comprehend the evidence I have to offer.

LETTER No. I.

To Dr. Pearfon, from C. D. Willaume, Efq.

My dear Sir,

I received the inclofed laft Saturday, and hope the anfwers to your queries will be fatisfactory, and tend to elucidate this curious fubject. Though the anfwers under the article *duft* only relate to your queries, yet my brother has thought proper to advert to the *afhes,* which you conceive to be a *caput mortuum;* but which have been ufed as, and have been fuppofed to be, a beneficial manure from time immemorial. I have referved a piece of the peat from which the afhes are produced, and if you would wifh to analyfe it, I will fend it you. Favour me with the refult of your future inquiries on this fubject, and I am,

Letter of information concerning peat afhes or duft.

My dear Sir, your's very fincerely,

Walham Green, C. D. Willaume.
Aug. 24, 1801.

LETTER No. II.

From John W. Willaume, Efq. to C. W. Willaume, Efq.

Queries propofed by Dr. Pearfon.

Queries refpecting falt of peat.

1. How long has the falt of peat been ufed?
2. How much per acre is laid on?
3. On what kind of lands?
4. The effects of it on vegetation?
5. Whether it is mixed with dung manure, or lime?
6. In what parts of the country has it been employed?
7. Any other facts which can be collected relative to the ufe of this fubftance?

In anfwering the above queries, I fhall divide the fubject into three articles, 1ft, the *peat* confidered as an object of fuel; 2d, the *afhes*; 3d, the *falt of peat, or duft;* the two laft as objects of manure.

1. *Peat*

Defcription of peat.

1. *Peat.* The peat, which is found after the removal of the turf or exterior furface, to about a fpade's depth, has long been known as an article of fuel. It is, however, ufed only by cottagers, who burn it on a brick hearth; it has been rejected from the parlour, the kitchen, the brewhoufe, &c. as being injurious to grates, and to all forts of veffels put on it; it cannot be employed in the roafting of meat, as it will impart a difagreeable tafte, and it is deftructive of all forts of furniture by the effluvia which it emits, or by the duft or afhes which may chance to be blown from it. If thefe difagreeable confequences could be obviated, it might be made an article of general confumption as a fubftitute for coal, much to the advantage of the feller and confumer; it is dug out in the form of a brick to a certain depth, well known to the common labourer. This depth muft be carefully attended to, left you fhould cut out the ftaple, in which cafe it would never be retrieved; but, this circumftance attended to, it will grow again to its former ftate in the fpace of fifteen years. Thus the whole moor is divided into proper portions, and periodically cut once in fifteen years.

Its afhes.

2. *Afhes.* The turf or furface, and fuch parts of the peat as do not appear to be of the beft quality, are laid up in confiderable heaps, and reduced to afhes by the action of fire. *The afhes are red.*

Anfwer to Queries.

Anfwers to the queries as to the afhes.

1. The afhes have been long known as a manure, and the demand is on the increafe.

2. The quantity ufually laid on an acre, by fpreading or fowing it, is fifty bufhels, either on grafs or arable land.

3. It is laid on hot land. By hot land, we underftand fandy, gravelly, chalky foils of a dry nature, fuch as are burnt up on the long continuance of hot weather. It is moft commonly ufed for graffes; but is in confiderable efteem, as a manure, for oats or barley on land of the nature above-mentioned.

4. The vegetable effect is furprifing, inafmuch as it will double or treble a crop of any new fown grafs, fuch as trefoil, &c. I have feen the benefits arifing from it on old pafture land much overgrown with mofs, which it effectually deftroys, and produces in its ftead white or Dutch clover. You may

trace

trace to an inch the ceſſation and recommencement of this manure. It is obſervable, that near the fire heaps, as far as the wind can carry the lighter parts of the aſhes, the production of clover is ſure to be abundant; it is equally favourable to the growth of barley or oats.

5. It is not mixed with lime, or any other manure.

6. Theſe aſhes are bought by a ſet of higlers, who carry them in bags loaded on aſſes to a conſiderable diſtance, where they are known to be in great repute; they muſt come exceſſively dear to the conſumer by this mode of conveyance. The farmers in the vicinity ſend for them in waggons, particularly Mr. Brumiger, near Sundon in Bedfordſhire, a conſiderable and intelligent farmer, who increaſes his conſumption every year, both for his graſs and arable land.

3. The Salt of Peat, or Duſt.

Anſwer to Queries.—1. The duſt or gray ſaline ſubſtance is produced by beating the earth containing this ſalt to a powder; it is found in particular ſpots, not univerſally, the earth not being equally impregnated with it in all places; it has not been known as a manure above ſix years; but on trial greatly increaſes in reputation and demand. Anſwers reſpecting the ſalt of peat.

2. Fifty buſhels are the proper quantity per acre. This ſhould not be exceeded, for if it be laid on in too great abundance, it may prove extremely deleterious.

3. It is uſed for cold lands. By cold lands we underſtand clayey, or any wet grounds.

4. It will much improve the vegetation of ſowed graſſes, and old paſture, and is equally favourable to the production of corn; the ground, whether graſs or arable, being of a cold nature.

5. It is not mixed with lime, or any other ſubſtance.

6. The duſt is likewiſe bought by the higlers, and carried to great diſtances. The nearer farmers likewiſe ſend for the duſt in waggons, particularly Mr. Anſtie, of Dunſtable Houghton, and Mr. Smith, of Sundon, who hold this manure in great eſteem.

Your's, &c.

Tingrith, Aug. 19, 1801. J. W. WILLAUME.

Dr. Pearſon's Experiments, Obſervations, and Remarks on the Subſtance called Salt of Peat, or Duſt.

Dr. Pearſon's chemical examination of ſalt of peat or duſt.

1. It is a blackiſh gray, coarſe, and rather heavy powder. Has no ſmell; taſtes ſtrongly ſtyptic; readily diſſolves in the mouth; did not deliqueſce on expoſure to the air.

2. Diſſolves in four times its weight of water of the temperature of fixty degrees of Fahrenheit, and in twice its weight of boiling hot water, giving a pale green coloured ſolution, with a trifling ſediment, which is inſoluble in muriatic acid.

3. To the ſolution (2.) I added a little liquid pruſſiate of vegetable alkali in a perfectly neutral ſtate, which occaſioned immediately a moſt abundant precipitation of pruſſiate of iron; and this teſt was added gradually, till no further precipitation took place.

4. Into the decanted and filtrated fluid (3) was poured liquid cauſtic volatile alkali, but without inducing any change.

5. Into the ſame fluid (3) was poured liquid carbonate of vegetable alkali, which produced a ſcarcely perceivable cloudy appearance.

6. Into the ſolution (3) was dropped the aqueous ſolution of muriate of baryt, which occaſioned immediately a milky appearance.

7. To the ſolution (3) I added the oxalic acid, and turbidneſs enſued.

8. A little of the powdery ſubſtance, called the ſalt of peat, with concentrated ſulphuric acid, produced no emiſſion of fumes, nor ſmell.

9. The ſolution (2) with muriate of baryt, immediately grew thick and white as cream.

10. The ſolution (2) with carbonate of potaſh, depoſited a very copious greeniſh ſediment; and the ſame effect enſued with cauſtic volatile alkali.

11. The ſolution (2) with oxalic acid, gave inſtantly a very turbid bluiſh green precipitation.

The preceding experiments manifeſted that the *peat ſalt* conſiſts of *ſulphate of iron,* vulgarly called green vitriol of iron, mixed with a very minute proportion of ſilicious earth, and of lime united either to ſulphuric acid, or to carbonic acid. But the preſence of the earths magneſia and argill; the uncombined

bined alkalies; the uncombined acids; are by these experiments excluded. In short, the salt of peat is almost pure *sulphate of iron.*

Remarks.

1. The salt of peat is, I apprehend, deposited by evaporations which run over the moors, where it is found; and hence I should expect many of such waters to be strongly impregnated with it, and in many parts the soil to be tinged red and yellow by ochre. Very likely * on enquiry much iron pyrites will be found on, or near the moors.

Salt of peat how produced.

2. The quantity spread on land is said to be fifty bushels per acre, which I estimate at 2,250 pounds avoirdupoise; this will give near seven ounces and a half per square yard. If a larger quantity be applied, it is observed it will prove extremely deleterious. This is true also of every other manure, such as lime, alkaline salts, marine salt, nay, of the dung of animals: for if they be used in certain quantities, they *poison* plants, instead of promoting their growth. This is equally true in the animal kingdom; for there is not an article taken as food, or as seasoning, which is not a poison, if taken in certain quantities. A human creature may be poisoned or alimented by beef or pudding, according to the quantity of them taken into the stomach. He may be poisoned, or have digestion greatly assisted by salt, or pepper, according to their quantity. In brief, the vulgar notion of the term *poison* is erroneous: for by it is conceived that substances so called are in their nature positively destructive of life; but the truth is that the most virulent poisons are, in all reason and fact, only deleterious according to the quantity applied. White arsenic swallowed in the quantity of ten grains or less, will destroy life; but in the quantity of one-sixteenth of a grain, it is as harmless as a glass of wine; and further, in that dose is a remedy for inveterate agues.

Its quantity must be regulated as a manure:

and it is hurtful only by excess, as food and condiments also are.

From these considerations I conclude, that there is no admissible contradictory evidence to the testimonies for the fer-

* "This is," says Mr. Willaume, "exactly the fact. This sulphate of iron, the salt of peat, during the heat of the summer is frequently found in a chrystalized state, very white, and crackling under the feet; but is deliquescent in that form, and turns to its former dark colour when the air becomes moist."—*Note by Mr. J. W. Willaume.*

tillizing

tilizing effect of sulphate of iron, unless by such contravening evidence the quantity stated to be used exceed fifty bushels per acre; it being an established fact, that in certain proportions this metallic salt is a poison to plants.

This discovery of Mr. Willaume will, I think, give new light, so as to explain fully the *rationale* of the improvement of land by the burnt earth and ashes from paring and burning. It is usual to account for the effects of this process, by referring to supposed alkaline or other salts; but of these there is no evidence, nay, on trial I have not detected them, or at least not in any efficient quantity; but this I know, that such earth and ashes contain *oxide of iron*, and as I suspect of *manganese*; which from the analysis, and the effect of salt of peat, must now be admitted into the class of manures. This very communication of Mr. Willaume, affords evidence of the truth of this conjecture, for the *ashes of the peat which afford the salt* " have been long known as a manure, and the demand is on the increase:" of course, these ashes contain an unusual quantity of oxide of iron. A consequence of this reasoning is, that the burnt earth of soils will, *cæteris paribus*, fertilize in proportion to the oxide of iron it contains. Accordingly the ashes of the peat, says Mr. Willaume, have a surprising effect, they " will double or treble a crop of any new-sown grass, such as trefoil, &c." they are so beneficial, that in spite of the expence they are carried in bags by higlers to great distances. It would be extending this paper beyond the proposed limits, to reason at a greater length, and to make a further induction of facts: therefore I will close with asserting, that the more I contemplate the facts in Mr. Willaume's letter, the more evidence I perceive for the truth, that metallic salts, and metallic oxides in general, and salts and oxides of iron in particular, are manures, if applied in proper doses.

It is considered as effective upon the same principle as condiments or seasoners.

I do not think it is within the design of this paper to make observations on the answers to the 2d, 3d, 4th, 5th, and 6th queries, except, once for all desiring that it may be understood, that I consider the *salt of peat and the ashes of peat*, as operating in promoting vegetation analogous to seasoning, or condiments, taken with the food of animals; that is, analogous to mustard, cinnamon, ginger, &c. which are not of themselves at all or necessarily nutritious, but contribute to render other things nutritious, by exciting the action of the stomach and

and other organs of digeftion and affimulation. I have no doubt of the truth of the propofition, that no living thing, neither plant nor animal, can grow and live in a ftate of vifible action without conftant fupplies of *matter which has been alive;* in other words, *living* animals and vegetables can only live on *dead* animals and *dead* vegetables. No plant, nor animal has ever been known by experience, nor in the nature of things does it feem reafonable, that they can be nourifhed by mere water and pure air, as fome perfons have afferted.

I fhall make a few remarks on the other *two fubftances* which are the fubject of Mr. Willaume's letter.

2. *The Peat.*

The peat is a denfe mafs of vegetable matter for a certain depth, partly in a *dead* and partly in a *living* ftate, with which is mixed more or lefs earth, and in burning it affords fo much empyreumatic oil, as to give a difagreeable tafte to roafted provifions; hence, as we are told, it has been rejected from the kitchen. This fuel affords a vaft quantity of what the chemifts call *lignic acid;* hence it is rejected alfo from the parlour, as very deftructive to the grates. I beg to fuggeft that this lignic acid might be faved in burning the peat as fuel, and be ufed for various purpofes in manufactures; and the charred peat may be ufed in place of charcoal of wood. Probably too other ufeful products will be found, on examining the matters more accurately which are afforded by diftillation.

Chemical remark on peat.

3. *Afhes.*

If the peat were mere vegetable matter, the afhes afforded by it would be as trifling as thofe of wood; but fome parts of the moor contain fo much earth and oxide of iron, as to leave behind, on burning, a confiderable quantity of incombuftible matter; and fuch kind of peat, we are told, is not ufed as fuel; but, after burning, the refiduary matter is an efficacious manure, much more fo than is commonly afforded by paring and burning. The afhes are more red and more fertilizing than afhes of common turf, becaufe they contain more iron.

The fpontaneous fpringing up of white clover, in land manured with thefe afhes, is fimilar to the fpontaneous growth of this plant on heath land, which has been covered with lime to deftroy all its prefent vegetation; and this fact fhews that probably

The afhe peat produce clover.

Explanation.

bably thefe are feeds buried in the earth for many ages, which yet remain alive, but do not grow until expofed to the ftimuli of air, water calorific, and lifelefs animal, or vegetable matter.

(To be concluded in our next.)

XII.

Letter from Mr. ACCUM, *refpecting an Error ftated to exift in his Practical Chemiftry.*

To Mr. NICHOLSON.

SIR,

Compton-Street, Soho, Feb. 16, 1805.

Omiffion ftated to be made in Accum's chemiftry.

W. F. C. cenfures me in your Journal, No. 38, page 105, of not having given in my book on Practical Chemiftry "*The means of preparing either nitric acid, or nitrate of potafh, or rather having ftated them by implication, as incapable of being produced by art.*" The fidelity with which you have laid his remarks before the public, encourages me to hope, that with equal impartiality you will allow me to appear before the fame tribunal, in order to plead to this accufation NOT GUILTY. For the method of obtaining nitric acid from its conftituent principles, W. F. C. may read in the Vol. I. page 211.

In order to let the judicious readers who are not in poffeffion of the work, judge for themfelves, I beg leave to lay before them the method there pointed out, which literally runs thus:

Paffage where the fuppofed omiffion is found.

"*Take a barometer tube, the diameter of which is about $\frac{1}{6}$ part of an inch. Shut one of its extremities with a cork, through the middle of which paffes a fmall wire with a ball of metal at each end. Fill the tube with mercury and invert it into a bafon of this fluid. Throw up into this tube as much of a mixture of* 12 *parts of nitrogen gas, and* 87 *parts of oxigen gas as will fill* 3 *inches. Through this gas by means of the wire in the cork pafs a number of electric fparks; the volume of the gas gradually diminifhes, and in its place will be found nitrous acid.*" *

* *Nitrous acid* does not differ from *nitric acid,* in compofition, but merely by the admixture of nitrous gas. See the book under confideration, Vol. II. p. 288.

It

It appears that W. F. C. has not noticed theſe lines. The production of nitric acid by ſyntheſis, being here clearly ſtated, his animadverſion relative to that ſubject falls to the ground.

To exculpate myſelf from his further accuſation, namely, of not having noticed the production of nitrate of potaſh by artificial means, or rather having ſtated it by implication, will become equally erroneous, on reminding him, that the method of preparing this ſalt artificially is pointed out, Vol. II. page 287, thus, "*Nitrate of potaſh may be prepared by neutralizing carbonate of potaſh with nitric acid.*" Other explanations.

From what has been ſtated, it is obvious that W. F. C.'s remarks are not correct, for both the methods of obtaining nitric acid, and nitrate of potaſh are correctly given, and not by implication, as W. F. C. apprehends.

I have the honour to be, Sir,

Your's,

FREDERICK ACCUM.

XIII.

Proceſſes for obtaining a durable and ſuperior Lake from Madder. By Sir H. C. Englefield, *M. P. F. R. S.**

THE want of a durable red colour, which ſhould poſſeſs ſomething of the depth and tranſparency of the lakes made from cochineal, firſt induced me to try whether the madder root, which is well known to furniſh a dye leſs ſubject to change by expoſure to air, than any other vegetable colour, except indigo, might not produce ſomething of the colour I wanted. Introduction.

Several of the moſt eminent painters of this country have, for ſome time, been in the habit of uſing madder lakes in oil pictures: but the colours they poſſeſſed under this name were either a yellowiſh red, nearly of the hue of brick-duſt, or a pale pink opake, and without clearneſs or depth of tint, and quite unfit to be uſed in water-coloured drawing, which was the principal object of my ſearch. Madder lakes have been already uſed; but they were bad.

My firſt attempts were to repeat the proceſs given by Margraf, in the Memoirs of the Academy of Berlin: but the co- Margraf's proceſs affords a weak colour,

* From the Tranſactions of the Society of Arts, who voted the gold medal in honour of this diſcovery. 1804.

lour

lour produced by this mode was of a pale red, and very opake, although the eminent author of the procefs ftates the colour he produced to be that of "*le fang enflammé,*" which probably means a deep blood colour. It may, however, be obferved, that colours prepared with a bafis of alumine will appear much deeper when ground in oil than they do in the lump, the oil rendering the alumine nearly tranfparent. This advantage is however, loft in water-colours. On examining the refiduum of the madder root, after it had been treated in Margraf's method, it appeared tinged with fo rich a red, that it was obvious, that by far the greater part of the colour ftill remained in it, and that the moft powerful and beautiful part. To extract this, feveral ineffectual trials were made, which it would be ufelefs to enter into; but, on attentively examining the appearances which took place on infufing the madder in water, I began to fufpect that the red colouring matter was very little, if at all, foluble in water, and that it was only mechanically mixed with the water when poured on the root, and fufpended in it by the mucilage, with which the root abounds.

because moft of the colour remained in the root.

Water takes up little of the colour.

A very fmall quantity, therefore, can be obtained by any infufion or decoction, as the greater part finks down on the root, or remains with it on the fieve, or in the bag, through which the infufion or decoction is paffed to render it clear. I therefore was induced to try whether, by fome merely mechanical means, I could not feparate the colouring matter from the fibrous part of the root. In this attempt my fuccefs was fully equal to my hopes; and, after feveral trials, I confider the procefs I am now about to defcribe, as the moft perfect I have been able to difcover.

Whence mechanical preffure is advifable.

Procefs 1. Enclofe two ounces, troy weight, of the fineft Dutch madder, known in commerce by the name of crop madder, in a bag, capable of containing three or four times that quantity, and made of ftrong and fine calico. Put it into a large marble, or porcelain mortar, and pour on it about a pint of cold foft water. The Thames water, when filtered, is as good as can be ufed; it being very nearly as pure as diftilled water, at leaft when taken up a very little way above London. With a marble or porcelain peftle, prefs the bag ftrongly in every direction, and, as it were, rub and pound it as much as can be done without endangering the bag. The water will very foon be loaded with the colouring matter, fo

Procefs 1. Two ounces of madder were pounded in a bag with a pint of water;

as

as to be quite opake and muddy. Pour off the water, and add another pint of frefh water to the root, agitating and triturating it in the manner before defcribed; and repeat the operation till the water comes off the root very flightly tinged. About five pints of water, if well agitated and rubbed, will extract from the root nearly the whole of its colour; and if the refidual root be taken out of the bag and dried, it will be found to weigh not more than five drachms, apothecaries weight; its colour will be a kind of light nankeen, or cinnamon, and it will have entirely loft the peculiar odour of the root, and only retain a faint woody fmell.

The water when turbid with colour is poured off and another pint added; and rubbed as before:

Five fucceffive pints extract all the colour, leaving five drams of fibre.

The water loaded with the colouring matter, muft be put into an earthen or well-tinned copper, or, what is ftill better, a filver veffel, (for the ufe of iron muft be carefully avoided through the whole), and heated till it juft boils. It muft then be poured into a large earthen or porcelain bafon, and an ounce troy weight of alum diffolved in about a pint of boiling foft water, muft be poured into it, and ftirred until it is thoroughly mixed. About an ounce and a half of a faturated folution of mild vegetable alkali fhould be gently poured in, ftirring the whole well all the time. A confiderable effervefcence will take place, and an immediate precipitation of the colour. The whole fhould be fuffered to ftand till cold; and the clear yellow liquor may then be poured off from the red precipitate. A quart of boiling foft water fhould again be poured on it, and well ftirred. When cool, the colour may be feparated from the liquor by filtration through paper in the ufual way; and boiling water fhould be poured on it in the filter, till it paffes through of a light ftraw colour, and quite free from any alkaline tafte. The colour may now be gently dried; and when quite dry, it will be found to weigh half an ounce; juft a fourth part of the weight of the madder employed.

The coloured water is made boiling hot;

and one oz. of alum diffolved in a pint of boiling water added;

$1\frac{1}{2}$ oz. of mild veg. alk. afford a precipitate of colour.

Edulcoration, filtering and drying afford half an ounce of lake,

By analyfis, this colour poffeffes rather more than 40 per cent. of alumine. If lefs than an ounce of alum be employed with two ounces of madder, the colour will be rather deeper; but if lefs than three quarters of an ounce be ufed, the whole of the colouring matter will not be combined with alumine. On the whole, I confider the proportion of an ounce of alum to two ounces of madder, as the beft.

which contains two fifths of alumine.

Procefs 2. If, when the folution of alum is added to the water loaded with the colouring matter of the root, the whole be

Procefs 2. If the liquor of procefs 1. be fuffered to cool

and fettle after the alum was added it affords a dull red fediment.

be fuffered to ftand, without the addition of the alkali, a confiderable precipitation will take place, which will be of a dark dull red. The remaining liquor, if again heated, will, by the admiffion of the alkali, produce a rofe-coloured precipitate of a beautiful tint, but wanting in force and depth of tone.

and the remaining fluid if heated and precipitated by alkali, affords a fine but not folid lake. This is Watt's procefs.

This is the procefs recommended by Mr. Watt, in his Effay on Madder, in the *Annals de chymie, Tome* 7; and this latter colour is what may perhaps, with propriety, be called Madder Lake. But, although the lighter red may be excellent for many purpofes, yet I confider the colour produced by the union of the two colouring matters, as given in the firft procefs, as far preferable for general ufe, being of a very beautiful hue when ufed thin, and poffeffing unrivalled depth and richnefs either in oil or water, when laid on in greater body.

Lefs alum caufes the fecond precipitate to be lefs in quantity, but richer.

If but half an ounce of alum be added to the two ounces of the root, the firft precipitate will be nearly fimilar to that when an ounce is employed; but the fecond, or lake precipitate, will be lefs in quantity, and of a deeper and richer tint. In this cafe the whole of the colouring matter, as before obferved, is certainly not combined with the alumine; for, on adding more alum to the remaining liquor, a precipitate is obtained of a light purplifh red. In this procefs, when two ounces of madder and an ounce of alum are ufed, the firft precipitate has about 20 per cent. of alumine, and the fecond, or lake precipitate, about 53 per cent; but thefe proportions will vary a little in repetitions of the procefs.

Procefs 3. When the wafhing is performed by boiling inftead of cold water, the colour is neither fo good nor fo plentiful.

Procefs 3. If the madder, inftead of being wafhed and triturated with cold water, as directed in the foregoing procefs, be treated in exactly the fame manner with boiling water; the colour obtained will be rather darker, but fcarcely of fo good a tint; and the refiduum of the root, however carefully preffed and wafhed, will retain a ftrong purplifh hue; a full proof that fome valuable colour is retained in it, probably fixed in the woody fibre by the action of heat. Mr. Watt, in his excellent treatife on madder above mentioned, obferves, that cold water extracts the colour better than hot water; and I have reafon to fufpect, that a portion of that colouring matter, which produces the bright red pigment, diftinguifhed before by the name of Madder Lake, remains attached to the root, when acted on by boiling water.

Procefs 4.

Process 4. If to two ounces of madder, a pint of cold water be added, and the whole be suffered to stand for a few days (three or four days) in a wide-mouthed bottle, lightly corked, in a temperature of between 50° and 60°, and often shaken; a slight fermentation will take place, the infusion will acquire a vinous smell, and the mucilaginous part of the root will be in a great degree destroyed, and its yellow colour much lessened. If the whole be then poured into a calico bag, and the liquor be suffered to drain away without pressure, and then the root remaining in the bag be treated with cold water, &c. exactly as directed in the first process, the red colouring matter will quit the root with much greater ease than before fermentation. It will also be equal in quantity to that afforded by the first process, but of a much lighter red. This difference of tint appears to be owing to a destruction of a part of the lake by the fermentation of the root; for if the colours from the fermented root be obtained separate, as in Process 2, the first precipitate will not sensibly differ from that obtained from the unfermented madder, but the second, or lake will be of a very light pink. This process, then, is not to be recommended.

Process 4. If the madder be suffered to stand before washing for some days in cold water, the mucilage is destroyed by fermentation, and the colour more easily extracted; but it is not so good.

Spanish and Smyrna Madders.

Spanish Madder affords a colour of rather a deeper tone than the Dutch Madder, but it does not appear to be of so pure a red as the Zealand Crop Madder.

The Smyrna Madder is a very valuable root. The colour produced from it by Process 1, is of a deeper and richer tint than any I have obtained from the Dutch Madder. The quantity produced from two ounces, is only three drachms, twenty-four grains: but this is not to be wondered at; for as this madder is imported in the entire root in a dry state, and the Crop Madder of Zealand consists principally of the bark, in which probably the greatest part of the colouring substance resides, there is every reason to think that the Smyrna madder really contains a greater proportion of colour than the Zealand, in equal weights of the entire root.

Spanish madder affords a deeper but less pure colour than Dutch madder. Smyrna madder is better than Dutch.

The products of Process 2 prove, that the *lake* of the Smyrna madder is more abundant in quantity, and of a richer tone than that of the Dutch root; for, from two ounces of Dutch madder the first precipitate was two drachms, and the lake was two drachms and forty eight grains; whereas, from

 two

two ounces of the Smyrna root the firft precipitate was one drachm and twenty four grains, and the lake was two drachms and twenty-four grains. The proportion of the lake to the other colour is, therefore, much higher in the Smyrna, than in the Dutch root.

Frefh Madder.

The recent root is preferable.

The colour may be prepared from the recent root; and it will be of a quality equal, if not fuperior, to any other. The difficulty of procuring the frefh root has prevented me from making as many experiments on it as I could have wifhed. I procured, however, a fmall quantity of the beft roots packed in mofs from Holland, and the following procefs anfwered perfectly well.

Experiment according to procefs 1. with frefh madder;

very fuccefsful.

Eight ounces of the root having been firft well wafhed and cleaned from dirt of all kinds, were broken into fmall pieces, and pounded in a bell-metal mortar, with a wooden peftle, till reduced into an uniform pafte. This pafte being enclofed in a calico bag, was wafhed and triturated, as defcribed in the firft procefs, with cold water. About five pints feem to have extracted nearly the whole of the colour. To the water thus loaded with colour, and boiled as before, one ounce of alum, diffolved in a pint of boiling water, was added, and the alkali poured on the whole, till the tafte of the mixture was juft perceptibly alkaline. The colour thus obtained, when dry, was of a very beautiful quality.

It promifes great advantage to the arts.

The fuccefs of this experiment, which was twice repeated with the fame refult, has led me to hope, that it is not impoffible that the mode of obtaining the colour from the frefh root here defcribed, may be productive of advantages for more extenfive ufe than I had in view when firft I attempted to obtain a pigment from madder. Many tracts of land in this country are as well adapted to the growth of this valuable article, as the foil of Holland can be; and the cultivation of it, which has more than once been attempted to a confiderable extent, has been laid afide, principally from the expence attendant on the erection of drying-houfes and mills, and the great expence and nicety requifite for conducting the procefs of drying. But fhould the colour prepared in the mode juft defcribed, be found to anfwer the purpofes of the dyers and calico-printers, the procefs is fo eafy, and the apparatus required

For if the colour fhould anfwer for dying, &c. it would fave drying and carriage;

quired for it so little expenſive, that it might be in the power of any grower of the root to extract the colour: beſides which, another great advantage would be obtained; the colour thus ſeparated from the root, may be kept any length of time, without danger of ſpoiling, and its carriage would be only one fourth of that of the root. I am, moreover, thoroughly inclined to believe, that in the preſent mode of uſing the root, a very conſiderable part of the colour is left in it by the dyers; and, ſhould this prove to be the caſe, an advantage much greater than any hitherto adverted to, may ariſe from the proceſs here recommended.

may be kept for a long time,

and probably be more effective.

Should it be attempted to obtain the colour from the freſh root, on an extenſive ſcale, I ſhould recommend, that the root be firſt reduced to as uniform a pulp as poſſible, by grinding or pounding. To this purpoſe, it is probable that the cyder-mill would anſwer perfectly well; and its extreme ſimplicity is a great recommendation. For the purpoſe of trituration, bags of woolen, ſuch as are uſed in the oil-mills, would probably anſwer as well as calico, and they would be much cheaper and more durable. A large vat, with ſtampers, would be eaſily conſtructed, by thoſe who are converſant in mechanics, for the holding them and preſſing them in water; and when the colour was boiled and precipitated, the flues of the boilers might eaſily be formed into convenient drying tables, without any additional expenſe of fuel. The part of the proceſs, which I conſider as of the greateſt importance, and as being the eſſential advantage of my methods over all thoſe which have come to my knowledge, is the trituration or preſſing of the root in water; and I believe that the colouring matter of the root has not been hitherto conſidered as ſo nearly inſoluble in water, as I have reaſon to think it is.

Directions for eſtabliſhing a manufactory of madder colour.

It were much to be wiſhed, that in the preſent advanced ſtate of Chemiſtry, ſome ſkilful analyſer would inveſtigate the properties of this very uſeful root; in which perhaps it will be found, that there are three, if not four, different colouring ſubſtances. Such are the proceſſes and views, which I have thought it not improper to ſubmit to the conſideration of the Society of Arts, &c.

We want a good analyſis of madder.

I have only now to deſcribe the ſpecimens which accompany this paper; aſſuring the Society, that they have been all prepared by my own hands entirely, and that I am therefore reſponſible

The ſpecimens exhibited to the Society.

refponfible for their having been produced by the proceffes ftated, without the addition of any foreign matter whatever, excepting the cake ground up with gum, and the bladder of oil-colour, which were prepared from the colour which I gave him, by Mr. Newman, of Soho-Square, whofe fkill and fidelity are too well known to need any teftimony in their favour.

It may be proper to add, that all the colours produced from the Dutch madder were prepared from the fame parcel of crop madder, in order that the differences in them might proceed from the proceffes, and not from a variation in the qualities of the root, which, in different fpecimens, will produce different fhades of colour under the fame mode of treatment.*

1. Dutch madder, treated by procefs 1ft.
2. Ditto - - - - procefs 2d.
3. Ditto - - - - procefs 3d.
4. Ditto - - - - procefs 4th.
5. Dutch madder, two ounces; alum, half an ounce; treated by procefs 2.
6. Dutch madder, two ounces; alum, one ounce; fermented two days, and then treated by procefs 2.
7. Produce of procefs 1, ground in gum by Mr. Newman.
8. Produce of procefs 1, ground in oil by Mr. Newman.

S—1. Smyrna madder, by procefs 1.
S—2. Ditto - - - procefs 2.
S—3. Ditto - - - procefs 3.
S—4. Ditto - - - procefs 4.

* Certificates accompanied the foregoing defcription, from Mr. Cotman and Mr. Munn, teftifying the merits of Sir H. Englefield's madder lakes, as water colours; and alfo, from Meffrs. Weft, Trumbull, Opie, Turner, Daniel, and Hoppner, fpeaking greatly iu its favour, where it has been tried in oil-colours.

The

XIV.

The Dutch Method of curing Herrings, extracted and tranſlated from the German of Krünitz's Economical Encyclopædia (Oeconomiſche Encyclopädie), *Article Häring, by* J. Hinckley, *Eſq. F. S. A.* *

THE veſſels employed in this fiſhery, commonly called herring-buſſes, from the Dutch name, are generally between 48 and 60 tons burthen, though ſome from 40 to 80 and 100 tons are uſed. The largeſt of all carry 120 tons, are three-maſted veſſels, with one deck, and a cabin at each end; that a-head ſerving as a kitchen. Of the larger, the crews are 24 men, thoſe of the ſmaller 18. They carry a few ſmall guns and muſquetry.

Dutch method of catching and curing of herrings.

Their nets are dipped, or caſt out, in the evening, and drawn up in the morning. It requires three hours to wind them on board. From the net, the fiſh are immediately put into baſkets, while others of the crew are occupied till evening in gutting, ſalting, and packing. But although from ten to fifteen laſt are ſometimes taken at a draught, the twelve perſons uſually employed for the purpoſe, cannot complete more than five laſt in a day.

During the three firſt weeks, from the 25th of June to the 16th of July, all the freſh-caught herrings are thrown into caſks without picking, and conveyed to Holland, in the jägers, or yachts †, that accompany the herring-buſſes. But, after this period, immediately on being got on board and gutted, they are aſſorted into three qualities,—maiden her-

* From the Tranſactions of the Society of Arts, who have publiſhed it for the information of our fiſhers in North Britain, who appear to be well acquainted with the treatment from catching and landing, but not with the ſubſequent proceſſes. Mr. Walter Baine, of Greenock, had the Society's ſilver medal for this object laſt ſeaſon.

† Theſe are ſmall faſt-ſailing veſſels, which follow the herring-buſſes, ſupply them with proviſions, caſks, ſalt, and other neceſſaries, and carry the fiſh that have been taken to the neareſt port, where they are re-packed, and prepared for ſending to the places of their deſtination.

rings,

Dutch method of catching and curing of herrings.

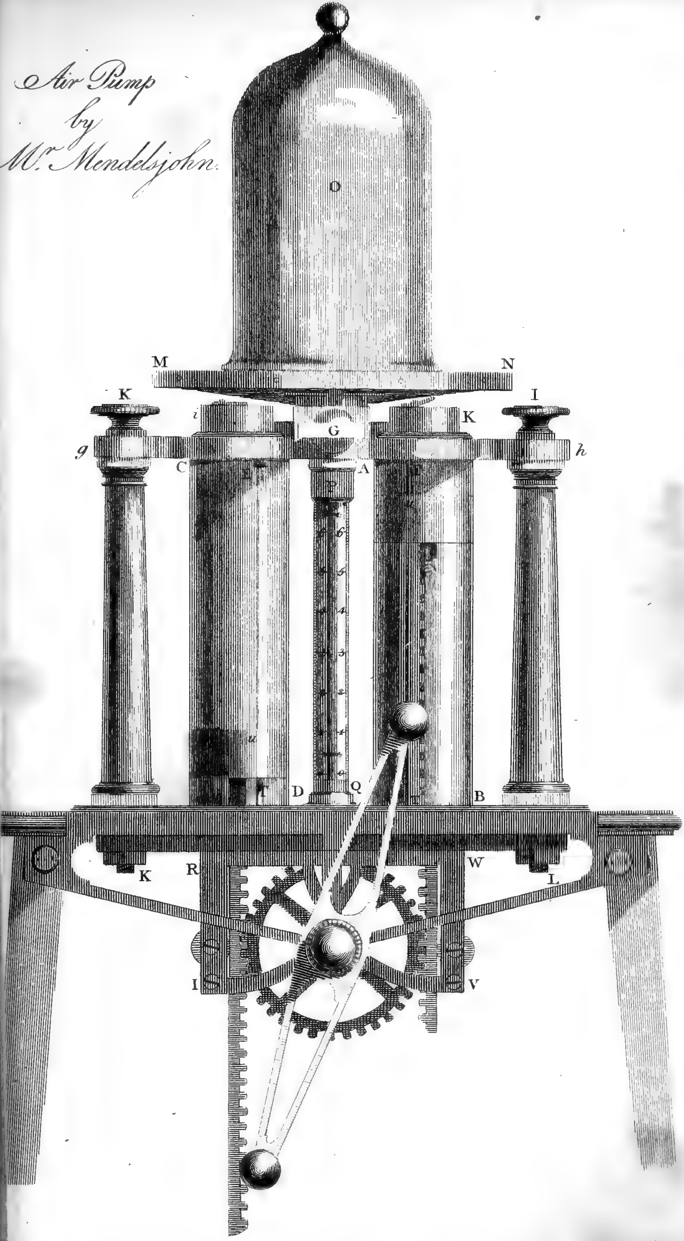
rings, full herrings, and ſhot herrings. The firſt of theſe are thoſe taken earlieſt, and without row or melt, but which, though well flavoured, do not keep. Full herrings are thoſe taken at Midſummer, on the point of ſpawning; from which the brand-herrings, ſo called from the barrels being marked with a hot iron, only differ in being caught later, re-packed immediately on arrival, and ſo cloſe and hard preſſed down, that they do not require re-packing at other places, but only new pickle; and are immediately expedited, or may remain on hand: whereas the other two ſorts, not being ſo cloſely laid, muſt abſolutely be re-packed. Shot-herrings are thoſe which have ſpawned, or are taken in the act of ſpawning, in conſequence of which they are thin and lean.

With the laſt two ſorts the buſſes themſelves return (as ſoon as they have got their loading, or find no more fiſh), one after another, to port, where all three ſorts, except the brand-herrings, before being expedited, are opened, ſalted anew, re-packed, and ſo heaped up, that fourteen caſks are re-packed in twelve, which make a laſt. By a regulation of the States-General, this re-packing muſt be performed in the open air, where ſtrict watch is kept, that the ſpoiling fiſh be carefully ſeparated from the good, and the latter properly laid in the barrels, and ſtrongly preſſed down.

The Dutch fiſhery continues generally from twenty to twenty-ſix weeks, or even ſomewhat longer, namely, from the 25th of June to the middle of January. The Dutch fiſh only on the Scotch and Engliſh coaſts, off Hittland, Fairhill, and Bocken, from Midſummer till the 25th of July; off Bocken or Serenial, from thence till the 14th of September; and in deep water, Eaſt of Yarmouth, and as far as the mouth of the Thames, from thence to the 25th of November, when the regular fiſhery ceaſes. But herrings are found not far from Yarmouth till the end of January, after which the fiſhery is prohibited, as the ſpawning ſeaſon then commences.

(To be continued.)

Air Pump
by
Mr. Mendelsjohn.



Mutlow Sculpsit

Mr. Gough's Theory of the Speaking Trumpet.

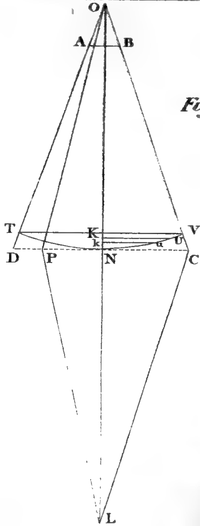


Fig. 3.

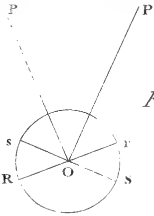


Fig. 4.

Part of the Air Pump.

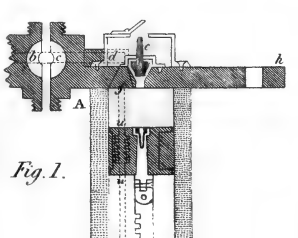


Fig. 1.

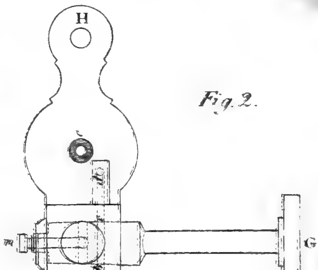


Fig. 2.

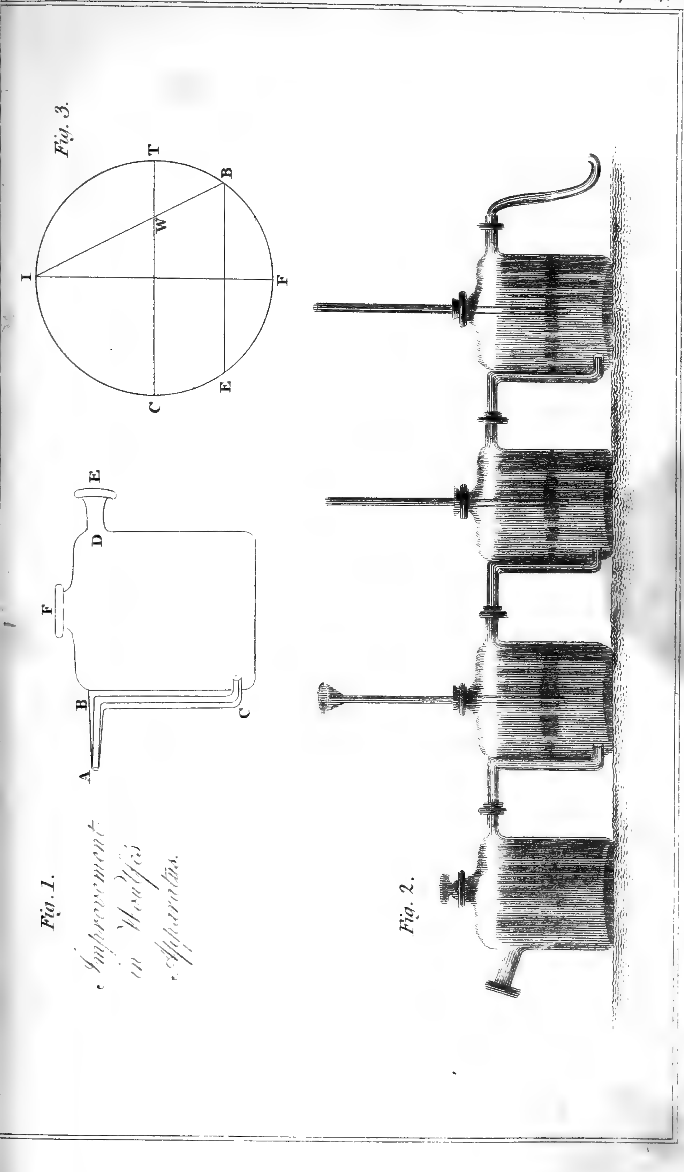


Fig. 1.
Improvement in Woulfe's Apparatus.
A
B
C
D
E
F
Fig. 2.
Fig. 3.
I
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B
W
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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1805.

ARTICLE I.

A ſhort Account of the Cauſe of the Diſeaſe in Corn, called by Farmers, the Blight, the Mildew and the Ruſt. Reprinted, and the Plates copied, with Permiſſion, from a Memoir communicated by the Right Hon. SIR JOSEPH BANKS, *Bart. P. R. S. &c. &c. &c. With ſome additional Notes.**

BOTANISTS have long known that the Blight in Corn is occaſioned by the growth of a minute paraſitic fungus or muſhroom on the leaves, ſtems, and glumes of the living plant. Felice

Botaniſts have long known that the blight is cauſed by a minute paraſitic fungus;

* Immediately after the title in the original is the following prefatory note:

" The following brief Publication, ſuggeſted by the alarming " ſtate of the harveſt in Auguſt laſt, would have been diſtributed " before the end of wheat ſeed-time, had the engraver fulfilled his " engagement.

" This circumſtance will, it is hoped, be conſidered as a ſufficient " apology for the want of actual obſervations on the origin and pro- " greſs of the diſeaſe. Theſe it is preſumed will be abundantly " ſupplied in the courſe of the preſent year, by thoſe intelligent " agriculturiſts, whoſe reſidence in the country enables them daily " to examine, not only the progreſs of their crops, but the origin " and advances alſo of all thoſe obſtacles which nature has oppoſed to the ſucceſs of agricultural labours, as if to awaken the ener- " gies

Felice Fontana publifhed in the year 1767 an elaborate account of this mifchievous weed,* with microfcopic figures, which give a tolerable idea of its form; more modern botanifts † have given figures both of corn and of grafs affected by it, but have not ufed high magnifying powers in their refearches.

but agriculturifts have paid little attention to this fact.

Agriculturifts do not appear to have paid, on this head, fufficient attention to the difcoveries of their fellow-labourers in the field of nature; for though fcarce any Englifh writer of note on the fubject of rural economy has failed to ftate his opinion of the origin of this evil, no one of them has yet attributed it to the real caufe, unlefs Mr. Kirby's excellent papers on fome difeafes of corn, publifhed in the Tranfactions of the Linnæan Society, are confidered as agricultural effays.

Engravings of this deftructive plant from accurate drawings by Mr. Bauer are here offered to their confideration.

On this account it has been deemed expedient to offer to the confideration of farmers, engravings of this deftructive plant, made from the drawings of the accurate and ingenious Mr. Bauer, Botanical Painter to his Majefty, accompanied with his explanation, from whence it is prefumed an attentive reader will be able to form a correct idea of the facts intended to be reprefented, and a juft opinion whether or not they are, as is prefumed to be the cafe, correct and fatisfactory.

Organized ftructure of the furface of ftraw. Its pores or mouths, fhut in dry and open in wet weather.

In order, however, to render Mr. Bauer's explanation more eafy to be underftood, it is neceffary to premife, that the ftriped appearance of the furface of a ftraw which may be feen with a common magnifying glafs, is caufed by alternate longitudinal partitions of the bark, the one imperforate, and the other furnifhed with one or two rows of pores or mouths, fhut in dry, open in wet weather, and well calculated to imbibe fluid whenever the ftraw is damp. ‡

By

"gies of reafon, and to reward the farmer for the exertions of his "intellectual faculties, by the fatisfaction of furmounting them."

"Jan. 30th, 1805." "JOS. BANKS."

* Offervazioni fopra la Ruggine del Grano. Lucca, 1767, 8vo.

† Sowerby's Englifh Fungi, Vol. II. Tab. 140, Wheat Tab. 139. Poa aquatica.

The pores or mouths on the furface of plants are defigned to abforb moifture.

‡ Pores or mouths fimilar to thefe are placed by nature on the furface of the leaves, branches, and ftems, of all perfect plants, a provifion intended no doubt to compenfate, in fome meafure, the want of loco-motion in vegetables. A plant cannot when thirfty

go

By these pores, which exist also on the leaves and glumes, it is presumed that the seeds of the fungus gain admission, and at the bottom of the hollows to which they lead, (see Plate IX. Fig. 1, 2,) they germinate and push their minute roots, no doubt (though these have not yet been traced) into the cellular texture beyond the bark, where they draw their nourishment, by intercepting the sap that was intended by nature for the nutriment of the grain; the corn of course becomes shrivelled in proportion as the fungi are more or less numerous on the plant; and as the kernel only is abstracted from the grain, while the cortical part remains undiminished, the proportion of *flour to bran* in blighted corn, is always reduced in the same degree as the corn is made light. Some corn of this year's crop will not yield a stone of flour from a sack of wheat; and it is not impossible that in some cases the corn has been so completely robbed of its flour by the fungus, that if the proprietor should choose to incur the expense of thrashing and grinding it, bran would be the produce, with scarce an atom of flour for each grain.

The seeds of the fungus enter the pores,

germinate in the cellular texture, intercept the sap, and render the grain shriveiled by abstraction of the kernel.

The flour may thus be almost totally intercepted.

Every species of corn, properly so called, is subject to the blight; but it is observable that spring corn is less damaged by it than winter, and rye less than wheat, probably because it is ripe and cut down before the fungus has had time to increase in any large degree.—Tull says that "white cone or "bearded wheat, which hath its straw like a rush full of pith, "is less subject to blight than Lammas wheat, which ripens a "week later." See page 74. The spring wheat of Lincolnshire was not in the least shrivelled this year, though the straw was in some degree infected: the millers allowed that it was the best sample brought to market. Barley was in some places considerably spotted, but as the whole of the stem of that grain is naturally enveloped in the hose or basis of the leaf, the fungus can in no case gain admittance to the straw; it is how-

All corn is liable to the blight; spring corn less than winter, and rye less than wheat.

Historical facts,

go to the brook and drink, but it can open innumerable orifices for the reception of every degree of moisture, which either falls in the shape of rain and of dew, or is separated from the mass of water always held in solution by the atmosphere; it seldom happens in the driest season, that the night does not afford some refreshment of this kind, to restore the moisture that has been exhausted by the heats of the preceding day.

ever

ever to be obferved that barley rifes from the flail lighter this year than was expected from the appearance of the crop when gathered in.

Little information was obtained by enquiry refpecting blight; but its caufe being now explained, the remedy it is hoped will be difcovered.

Though diligent enquiry was made during the laft autumn, no information of importance relative to the origin or the progrefs of the blight could be obtained: this is not to be wondered at; for as no one of the perfons applied to had any knowledge of the real caufe of the malady, none of them could direct their curiofity in a proper channel. Now that its nature and caufe have been explained, we may reafonably expect that a few years will produce an interefting collection of facts and obfervations, and we may hope that fome progrefs will be made towards the very defirable attainment of either a preventive or a cure.

Progrefs of the infection.

It feems probable that the leaf is firft infected in the fpring, or early in the fummer, before the corn fhoots up into ftraw, and that the fungus is then of an orange colour;* after the ftraw has become yellow, the fungus affumes a deep chocolate brown: each individual is fo fmall that every pore on a ftraw will produce from 20 to 40 fungi, as may be feen in the plates, and every one of thefe will no doubt produce at leaft 100 feeds; if then one of thefe feeds tillows out into the number of plants that appear at the bottom of a pore in *Plate* IX. *Fig.* 1, 2. how incalculably large muft the increafe be! A few difeafed plants fcattered over a field muft very fpeedily infect a whole neighbourhood, for the feeds of fungi are not much heavier than air, as every one who has trod upon a ripe puff-ball muft have obferved by feeing the duft, among which is its feed, rife up and float on before him.

Increafe of the fungus incalculably rapid;

and its periods of generation very quick,

How long it is before this fungus arrives at puberty, and fcatters its feeds in the wind, can only be gueffed at by the analogy of others; probably the period of a generation is fhort, poffibly not more than a week in a hot feafon: if fo, how frequently in the latter end of the fummer muft the air be loaded as it were with this animated duft, ready, whenever a

* The Abbé Teffier, in his *Traité des Maladies des Grains*, tells us that, in France, this difeafe firft fhews itfelf in minute fpots of a dirty white colour on the leaves and ftems, which fpots extend themfelves by degrees, and in time change to yellow, and throw off a dry orange coloured powder. pp. 201, 340. *Note of Sir J. B*

gentle

gentle breeze, accompanied with humidity, ſhall give the ſignal to intrude itſelf into the pores of thouſands of acres of corn. Providence, however, careful of the creatures it has created, has benevolently provided againſt the too extenſive multiplication of any ſpecies of being; was it otherwiſe, the minute plants and animals, enemies againſt which man has the feweſt means of defence, would increaſe to an inordinate extent; this however, can in no caſe happen, unleſs many prediſpoſing cauſes afford their combined aſſiſtance. But for this wiſe and beneficent proviſion, the plague of ſlugs, the plague of mice, the plagues of grubs, wire-worms, chafers, and many other creatures whoſe power of multiplying is countleſs as the ſands of the ſea, would, long before this time, have driven mankind, and all the larger animals, from the face of the earth.

If there were no proviſion in nature to check the increaſe of ſmall plants and animals, the larger would ſpeedily be deſtroyed by them.

Though all old perſons who have concerned themſelves in agriculture remember the blight in corn many years, yet ſome have ſuppoſed that of late years it has materially increaſed; this however does not ſeem to be the caſe. Tull, in his Horſe-hoeing Huſbandry, p. 74, tells us, that the year 1725 "was a "year of blight the like of which was never before heard of, and "which he hopes may never happen again;" yet the average price of wheat in the year 1726, when the harveſt of 1725 was at market, was only 36*s.* 4*d.* and the average of the five years of which it makes the firſt, 37*s.* 7*d.*—1797 was alſo a year of great blight; the price of wheat in 1798 was 49*s.* 1*d.* and the average of the five years, from 1795 to 1799, 63*s.* 5*d.**

The blight does not appear to have increaſed of late years.

The climate of the Britiſh Iſles is not the only one that is liable to the blight in corn; it happens occaſionally in every part of Europe, and probably in all countries where corn is grown.

The blight in corn probably happens in all countries.

* The ſcarcity of the year 1801 was in part occaſioned by a mildew which, in many places, attacked the plants of wheat on the S.E. ſide only: but it was principally owing to the very wet harveſt of 1800. The deficiency of wheat, at that harveſt, was found, on a very accurate calculation, ſomewhat to exceed one fourth. But wheat was not the only grain that failed: all others, and potatoes alſo were materially deficient. This year the wheat is probably ſomewhat more damaged than it was in 1800, and barley ſomewhat leſs than an average crop. Every other article of agricultural food is abundant, and potatoes one of the largeſt crops that has been known. But for theſe bleſſings on the labour of man, wheat muſt before this time have reached an exorbitant price.—*Sir J. B.*

Italy is very ſubject to it, and the laſt harveſt of Sicily has been materially hurt by it. Specimens received from the colony of New South Wales ſhew that conſiderable miſchief was done to the wheat crop there in the year 1803 by a paraſitic plant, very ſimilar to the Engliſh one.

Common opinion that wheat in the neighbourhood of barberries is moſt frequently blighted.

It has been long admitted by farmers, though ſcarcely credited by botaniſts, that wheat in the neighbourhood of a barberry buſh ſeldom eſcapes the blight. The village of Rolleſby in Norfolk, where barberries abound, and wheat ſeldom ſucceeds, is called by the opprobrious appellation of Mildew Rolleſby. Some obſerving men have of late attributed this very perplexing effect to the farina of the flowers of the barberry, which is in truth yellow, and reſembles in ſome degree the appearance of the ruſt, or what is preſumed to be the blight in its early ſtate.

The barberry leaf is very ſubject to a mildew fungus;

It is, however, notorious to all botanical obſervers, that the leaves of the barberry are very ſubject to the attack of a yellow paraſitic fungus, larger, but otherwiſe much reſembling the ruſt in corn.

which may be transferred to corn.

Is it not more than poſſible that the paraſitic fungus of the barberry and that of wheat are one and the ſame ſpecies, and that the ſeed transferred from the barberry to the corn is one cauſe of the diſeaſe. Miſletoe, the paraſitic plant with which we are the beſt acquainted, delights moſt to grow on the apple and hawthorn, but it flouriſhes occaſionally on trees widely differing in their nature from both of theſe: in the Home Park, at Windſor, miſletoe may be ſeen in abundance on the lime trees planted there in avenues. If this conjecture is founded, another year will not paſs without its being confirmed by the obſervations of inquiſitive and ſagacious farmers.

Conjectures directed to remedy.

It would be preſumptuous to offer any remedy for a malady, the progreſs of which is ſo little underſtood; conjectures, however, founded on the origin here aſſigned to it, may be hazarded without offence.

Probable fact as to its earlieſt appearance.

It is believed * to begin early in the ſpring, and firſt to appear on the leaves of wheat in the form of ruſt, or orange-coloured powder; at this ſeaſon, the fungus will, in all probability,

* This, though believed, is not dogmatically aſſerted, becauſe Fontana, the beſt writer on the ſubject, aſſerts that the yellow and the dark-coloured blight are different ſpecies of fungi.

bability,

bability, require as many weeks for its progrefs from infancy to puberty as it does days during the heats of autumn; but a very few plants of wheat, thus infected, are quite fufficient if the fungus is permitted to ripen its feed, to fpread the malady over a field, or indeed over a whole parifh.

The chocolate-coloured blight is little obferved till the corn is approaching very nearly to ripenefs; it appears then in the field in fpots, which increafe very rapidly in fize, and are in calm weather fomewhat circular, as if the difeafe took its origin from a central pofition.

The chocolate coloured blight feems to fpread from central points of the field.

May it not happen then, that the fungus is brought into the field in a few ftalks of infected ftraw uncorrupted among the mafs of dung laid in the ground at the time of fowing? it muft be confeffed, however, that the clover lays, on which no dung from the yard was ufed, were as much infected laft autumn as the manured crops. The immenfe multiplication of the difeafe in the laft feafon, feems however to account for this; as the air was no doubt frequently charged with feed for miles together, and depofited it indifcriminately on all forts of crops.

Whence it may have been brought in by infected ftraw in the manure;

It cannot however be an expenfive precaution to fearch diligently in fpring for young plants of wheat infected with the difeafe, and carefully to extirpate them, as well as all graffes, for feveral are fubject to this or a fimilar malady, which have the appearance of orange-coloured or of black ftripes on their leaves, or or their ftraw; and if experience fhall prove that uncorrupted ftraw can carry the difeafe with it into the field, it will coft the farmer but little precaution to prevent any mixture of frefh ftraw from being carried out with his rotten dung to the wheat field.

and may, it is likely, be guarded againft by extirpating the plants firft infected.

In a year like the prefent, that offers fo fair an opportunity, it will be ufeful to obferve attentively whether cattle in the ftraw yard thrive better or worfe on blighted than on healthy ftraw. That blighted ftraw, retaining on it the fungi that have robbed the corn of its flour, has in it more nutritious matter than clean ftraw which has yielded a crop of plump grain, cannot be doubted; the queftion is, whether this nutriment in the form of fungi does, or can be made to agree as well with the ftomachs of the animals that confume it, as it would do in that of ftraw and corn.

Whether blighted ftraw be more or lefs nutritious than clean ftraw, for cattle?

It

Though the ſeeds of blighted corn afford little flour, they are each as good for ſowing as the plumpeſt ſamples;

It cannot be improper in this place to remark, that although the ſeeds of wheat are rendered, by the exhauſting power of the fungus, ſo lean and ſhrivelled that ſcarce any flour fit for the manufacture of bread can be obtained by grinding them, theſe very ſeeds will, except, perhaps, in the very worſt caſes *, anſwer the purpoſe of ſeed corn as well as the faireſt and plumpeſt ſample that can be obtained, and, in ſome reſpects better; for as a buſhel of much blighted corn will contain one-third at leaſt more grains in number than a buſhel of plump corn, three buſhels of ſuch corn will go as far in ſowing land, as four buſhels of large grain.

and are much more numerous in an equal meaſure.

The flour in the ſhrivelled grain is amply ſufficient to nouriſh the minute plants till they take root.

The uſe of the flour of corn in furthering the proceſs of vegetation, is to nouriſh the minute plant from the time of its developement till its roots are able to attract food from the manured earth; for this purpoſe one-tenth of the contents of a grain of good wheat is more than ſufficient. The quantity of flour in wheat has been increaſed by culture and management calculated to improve its qualities for the benefit of mankind, in the ſame proportion as the pulp of apples and pears has been increaſed by the ſame means, above what is found on the wildings and crabs in the hedges.

It is a waſteful practice to take the plumpeſt grain for ſeed. The ſmalleſt though unfit for the mill are equally good for this uſe.

It is cuſtomary to ſet aſide or to purchaſe for ſeed corn, the boldeſt and plumpeſt ſamples that can be obtained; that is, thoſe that contain the moſt flour; but this is unneceſſary waſte of human ſubſiſtence; the ſmalleſt grains, ſuch as are ſifted out before the wheat is carried to market, and either conſumed in the farmer's family, or given to his poultry, will be found by experience to anſwer the purpoſe of propagating the ſort from whence they ſprung, as effectually as the largeſt.

Arrangement of the grains in an ear of wheat. The lower ones ſupply the moſt food, but the upper are not inferior as ſeed.

Every ear of wheat is compoſed of a number of cups placed alternately on each ſide of the ſtraw; the lower ones contain, according to circumſtances, three or four grains, nearly equal in ſize, but towards the top of the ear, where the quantity of nutriment is diminiſhed by the more ample ſupply of thoſe cups that are nearer the root, the third or fourth grain in a cup is frequently defrauded of its proportion, and becomes

* 80 grains of the moſt blighted wheat of the laſt year, that could be obtained, were ſown in pots in the hot-houſe; of theſe, ſeventy-two produced healthy plants, a loſs of 10 per cent. only

ſhriveled

fhrivelled and fmall. Thefe fmall grains, which are rejected by the miller, becaufe they do not contain flour enough for his purpofe, have neverthelefs an ample abundance for all purpofes of vegetation, and as fully partake of the fap, (or blood, as we fhould call it in animals,) of the kind which produced them, as the faireft and fulleft grain that can be obtained from the bottoms of the lower cups by the wafteful procefs of beating the fheaves.

EXPLANATION OF THE PLATES.

Plate IX.

Fig. 1. A piece of the infected wheat ftraw—natural fize: at *a* the leaf fheath is broken and removed, to fhew the ftraw which is not infected under it.

Fig. 2. A highly magnified reprefentation of the parafitic plant which infects the wheat: *a* in a young ftate; *b* full grown; *c* are two plants burfting and fhedding their feeds when under water in the microfcope; *d* two plants burft in a dry ftate; *e* feems to be abortive; *f* feeds in a dry ftate; *g* a fmall part of the bottom of a pore with fome of the parafitic fungi growing upon it.

Fig. 3. A part of the ftraw of *fig.* 1, magnified.

Fig. 4. Part of *fig.* 3 at *a b* more magnified.

Fig. 5. Part of a ftraw fimilar to *fig.* 3, but in its green ftate, and before the parafitic plant is quite ripe.

Fig. 6. A fmall part of the fame, more magnified.

Plate X.

Fig. 1. A highly magnified tranfverfe cutting of the ftraw, correfponding with *Fig.* 4. *Plate* I. fhewing the infertion of the parafite in the bark of the ftraw.

Fig. 2. A longitudinal cutting of the fame; magnified to the fame degree.

Fig. 3. A fmall piece of the epidermis of a ftraw, fhewing the large pores which receive the feed of the parafite; the fmaller fpots obfervable on the epidermis, are the bafes of hairs

hairs that grow on the plant of the wheat whilst young, but which fall off when it ripens, magnified to the same degree as the preceding figures.

II.

Theorems respecting the Properties of the Sides of Triangles intersected by Right Lines drawn from the three Angles so as to meet in one Point. By Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Middleshaw, March 1, 1805.

Certain properties of triangles.

GEOMETRICIANS have paid but little attention to the properties of the sides of triangles, which are intersected by right lines, drawn from the three opposite angles so as to meet in one point. Perhaps all the theorems of this kind that have been hitherto published, are contained in the second book of Emerson's Geometry, with the single exception of a proposition given by Mr. Landen; who has proved that if one of the internal angles of a triangle, and the two opposite external angles of the same be bisected, the right lines, drawn for this purpose, will also meet in a common point. The following propositions take a more general view of the subject; and as some mathematical papers have appeared of late in the Philosophical Journal, I have ventured to ask a place for the present sheet, in your valuable miscellany.

I remain, &c.

JOHN GOUGH.

P. S. In vol. X. page 66, line 2, for $5\frac{1}{2}$ yards read $3\frac{1}{2}$ yards.

Proposition 1*st*. Let three right lines C M, B N and A S, (*Plate* XI. *Fig*. 1, 2.) drawn from the three angles of a triangle A B C, intersect the opposite sides, produced or not, in the points M, N and S, and also meet each other in a common point O; then if all the points M, N and S lay betwixt the

the angular points B, A and C, the place of O will be within the triangle, and the three lines C O, B O and A O will fall wholly within the fame; but if one of thefe points, as S, lay in B E, which is C B produced, the place of O will be external to the triangle; in which cafe, one of the two remaining points of interfection will lay in its refpective fide produced; but the fituation of the other will always be betwixt the angular points of the third fide.

Certain properties of triangles.

Demonftration.

Cafe 1ft. Let M, N and S *(Fig. 1.)* be betwixt the angular points B, A and C; in which cafe, the triangles A B C, M B C are of the fame altitude; confequently they are as their bafes A B, B M, *Euc.* 1.6; but B M is a part of A B by hypothefis; therefore the triangle M B C is a part of A B C, *(Simpfon's Euclid.* D. 5.) that is, the triangle A B C contains the triangle M B C; for the fame reafon, the fame A B C contains the triangle S A B; but the figure M B S O is common to the triangles M B C, S A B; therefore it is contained in the triangle A B C; confequently the place of O is within the fame: now as the points A, B, C and O are all found in the fpace A B C, the lines A O, B O and C O muft alfo lay wholly within the fame triangle.

Cafe 2. When the point S falls in B E or C B produced, *(Fig.* 2.) the angle C B A is external to the triangle A B S; therefore it is greater than the angle B S A, *(Euc.* 17, 1;) and the fum of the angles C A B, A C B is lefs than the angles C A S, A C S taken together, *(Euc.* 32. 1); hence the angle C A B is lefs than C A S; confequently the right line A S, produced at pleafure, falls wholly without the triangle A B C. Now if the remaining two lines C M and B N be fuppofed to cut the fides B A and A C not produced, the place of O, their common interfection, will be in the fpace A B C, by *Cafe* 1: but this is impoffible; for O is in the right line A S by hypothefis; which has been fhewn to fall wholly without the triangle A B C, confequently one at leaft of the lines C M, B N muft alfo lay without the fame fpace; let this be B N, and draw O K parallel to C A; then the external angle O K E of the triangle O K C is greater than the internal and oppofite angle

Certain properties of triangles.

angle B C M, 17.1; but the angles O K E, A C B are equal (*Euc.* 29.1;) therefore the triangle M B C is a part of the triangle A B C, and is contained in it; but these triangles are as their bases M B, B A (*Euc.* 1.6;) consequently M B is a part of B A, Simf. *Euc.* D. 5; and the point M is between the points A and B. Q. E. D.

Corollary. Hence it appears that if two lines C M and B N be drawn, and their interfection O lay within the triangle A B C, the place of the third point S will be in the fide B C unproduced. But if O lay without the triangle, the place of the third point will depend on the fituation of the given points; namely if thefe be in two fides produced, the place fought will lay in the remaining fide unproduced; but if either of the given points fall between two angles of the triangle, the third will be found in the remaining fide produced.

Propofition 2. If a right line C M (*Fig.* 1.) be drawn from C, one of the angles of a triangle A B C, to cut the oppofite fide A B, produced or not, in M, and if from the point M, perpendiculars, M F, M H be let fall upon the fides A C, C B, the rectangle B M, M F will be to the rectangle A M, M H, as B C is to C A.

Demonftration. Draw A P, B Q perpendicular to B C, C A; then we have the following proportions, by femilar triangles; as M B : M H : : A B : A P; and as M F : M A : : B Q : B A; hence as M B × M F : A M × M H : : B Q : A P. But as B Q : A P : : B C : C A, *Euc.* 14.6; therefore the rectangle M B, M F is to the rectangle M A, M H as B C is to C A. *Euc.* 11.5. Q. E. D.

Cor. 1*ft.* If O *h*, O *f* be drawn perpendicular to B C, C A, from any point O in M C; the rectangle M B, O *f* will be to M A, O *h* as B C is to C A; for by fimilar triangles as *f* O : O *h* : : F M : M H; hence as M B × O *f* : M A × O *h* : : M B × M F : M A × M H; confequently as M B × O *f* : M A × O *h* : : B C : C A. *Euc.* 11.5.

Cor. 2*nd.* Let the triangle A B C be ifofceles, having the fides B A, A C equal; alfo let C M, B N, drawn to the oppofite fides B A, A C meet in O; then the point S is given, in which A O, produced if neceffary, cuts the remaining fide C B. For draw O *f*, O *g* perpendicular to C A, A B, and we fhall have, as B S × O *f* : C S × O *g* : : B A : A C, *Cor.* 1; but

B A,

B A, A C are equal by hypothefis; therefore $B S \times O f = C S \times O g$; hence as $O g : O f :: B S : S C$; confequently the point S may be found, proper attention being paid to *Cor. Prop.* 1. Certain properties of triangles.

Propofition 3. If three right lines C M, B N, A S, drawn from the three angles of a triangle A B C (*Fig.* 1 and 2) meet each other in a point O, and the oppofite fides B A, A C and C B, in M, N and S; the fegments of any one fide are as the rectangle under the alternate fegments of the remaining two fides; *i. e.* B S is to S C as the rectangle B M, A N to the rectangle C N, A M.

Demonftration. Draw $O f$, $O g$, $O h$ perpendicular to C A, A B and B C. Then as $B C : C A :: B M \times O f : A M \times O h$; *Cor.* 1. *Prop.* 2; for the fame reafon as $A B : B C :: N A \times O h : N C \times O g$; hence as $B A : A C :: M B \times N A \times O f : N C \times M A \times O g$; but as $B A : A C :: B S \times O f : C S \times O g$; confequently B S is to S C as the rectangle B M, A N is to the rectangle C N, A M. Q. E. D.

Cor. If two right lines C M, B N be drawn from two angles of any plain triangle A B C, fo as to meet each other in a point O, and the oppofite fides B A, A C in M and N; a point S may be found in the remaining fide, produced if neceffary, fo that if S A be joined, the right line A S fhall pafs through O. For fince the right lines B M, M A, A N and N C are given, the rectangles B M, A N and C N, A M are alfo given; confequently the ratio of thefe rectangles is given: but the right line A O cuts the fide C B, produced or not, according to circumftances, in the fame ratio, by the laft propofition; therefore if both M and N lay betwixt the angular points of the triangle, or in their refpective fides produced, divide B C in S, fo that B S may be to S C in the ratio of the rectangles B M, A N and C N, A M; then will S be the required point by *Cor.* to *Prop.* 1; but if M be fituated betwixt the angles B, A, and N lay in C A produced; find S in C B alfo produced, fo that B S may be to S C as the rectangle B M, A N is to the rectangle C N, A M; and S will be the required point, by the corollary laft referred to.

Prop. 4. Let three right lines C M, B N and A S (*Fig.* 1 and 2) drawn from the three angles of a triangle A B C, interfect each other in a common point O, and meet the oppofite

Certain properties of triangles.

fite fides B A, A C and C B, in the points M, N and S; if A N be to N C as A^pB is to B^pC; but B M be to M A as B^rC is to C^rA; then will B S be to S C as the product of B^pA into B^rC is to the product of B^pC into A^rM.

Demonftration. As A N : N C : : B^pA : B^pC; and as B M : M A : : B^rC : C^rA by hypothefis; hence as B M $\times$ A N : C N $\times$ A M : : $B^pA \times B^rC$: $B^pC \times C^rA$; but as B M $\times$ A N : N C $\times$ A M : : B S : S C; confequently B S is to S C as the product of B^pA into B^rC is to the product of B^pC into C^rA. *Euc.* 11. 5. Q. E. D.

Corollary 1ft. The preceding theorem is general; for the ratios of B M to M A, and of A N to N C can be expreffed by fome power or powers of the lines B C, C A and A B, B C, which contain the angles oppofite to the fides B A and A C; therefore if the points M, N and S be arranged according to the corollary to Propofition 1ft, and the fegments betwixt thefe points and the adjacent angles have the ratios affigned in the prefent theorem, right lines, drawn from M, N and S to the oppofite angles, will all meet in one point.

Cor. 2. When r and p are equal, B S is to S C as B^pA is to A^pC by the propofition; therefore if the fegments betwixt the points M, N, S and the adjacent angles are in the ratios of the adjacent fides raifed to any given power, right lines, drawn from M, N and S to the oppofite angles, will all meet in one point by *Cor. 1ft.*

Cor. 3rd. Let $p = o$; then as B S : S C : : B^oA : A^oC by the propofition; but B^oA : A^oC is the ratio of equality; therefore if M, N and S bifect the three fides of a triangle, lines drawn from them to the oppofite angles will meet in one point by *Cor. 2nd.*

Cor. 4. Let $p = 1$; alfo let M and N interfect the fides B A, A C not produced; then S lays between the angular points B, C, *Cor. Prop.* 1; and as B S : S C : : B A : A C, *Cor.* 2; therefore the right line S A bifects the angle B A C; confequently if three right lines bifect the three internal angles of a triangle, they fhall all meet in one point by the laft propofition.

Cor. 5. Let $p = 1$ as before; alfo fuppofe M to be in the fide B A not produced; but let N be without the triangle; then S is in C B produced, *Cor. Prop.* 1; but as B S : S C : :

5 B A

B A : A C, *Cor.* 2; therefore S A bisects the angle B A N. (*Simpson's Euclid.* .6 A); also as B M : M A : : B C : C A; therefore C M bisects the angle A C B, *Euc.* 3.6; consequently if two lines bisect any two of the external angles of triangles, and a third line bisect the internal angle opposite to these angles, these lines shall meet in one point by the last proposition.

Prop. 5. If B M be to M A as B^pC to C^pA; and h O, Of be perpendicular to B C and C A; h O is to Of as $B^{p-1}C$ to $C^{p-1}A$.

Demonstration. As $BM \times Of : AM \times Oh :: BC : CA$, *Cor.* 1. *Prop.* 2; and as $B^pC : A^pC :: BM : MA$ by hypothesis; therefore as $B^pC \times Of : A^pC \times Oh :: BC : CA$; consequently as $Oh : Of :: B^{p-1}C : C^{p-1}A$. Q. E. D.

III.

A Communication on the Use of Green Vitriol, or Sulphate of Iron, as a Manure; and on the Efficacy of paring and burning depending, partly, on Oxide of Iron. By GEORGE PEARSON, M. D. *Honorary Member of the Board of Agriculture, F. R. S. From a Communication made by him to the Board, and inserted in the fourth Volume of their Transactions.*

(Concluded from page 214.)

APPENDIX.

THE following facts, lately discovered by most respectable chemists, appear to be worth adding to the preceding memoir, as they serve to shew that other salts, besides sulphate of iron and certain earths, may be employed advantageously as manures, although like iron they have been esteemed deleterious to plants.

1. *Ashes of Pit Coal are a good Manure for Grass.*

My much valued friend, the Rev. William Gregor, of Grampound, on examination of the ashes of coal from Liverpool, found them to contain both sulphate of magnesia and sulphate of lime, especially the former, salt. I apprehend that these ashes also contain oxide of iron, or perhaps sulphate of

The ashes of pit coal are a good manure for grass.

of iron. These ashes, says Mr. Gregor, "*skeaded*" * over grass apparently produced good effects *notwithstanding the sulphate of magnesia,* which I was well assured they contained. See Nicholson's Journal, Vol. V. p. 225.

Sulphate of magnesia may not be hurtful to plants, tho' pure magnesia is.

From this observation of Mr. Gregor, it seems he is aware of the prevailing popular opinion, that sulphate of magnesia is not favourable to vegetation; and to reconcile his fact with the unfriendly nature of magnesia to plants, as discovered by Mr. Tennant, he observes that the effects of sulphate of magnesia may be very different from those of magnesia and carbonate of magnesia. I apprehend it is the magnesia (calcined magnesia) only which this learned chemist found hurtful to vegetables, as the discovery was made on the examination of Nottingly lime, which the farmers near Doncaster employ as a manure, while they reject the lime of their own neighbourhood. In the latter Mr. Tennant met with magnesia, and in the former none. See the account of this important discovery in the Philos. Transactions.

2. *The Earth from Ashes called Cinis, is a durable and efficacious Manure; by Professor Mitchill, of New York, one of the Representatives in Congress. Addressed to Dr. Pearson.*

Earth from ashes is a good manure.

Dr. Mitchill, in a letter addressed to me on *cinis*, or earth found in the ashes of wood, has made some observations relative to the preceding memoir, which seem worthy of notice.

"Ashes of wood contain very commonly sulphate of potash, also phosphoric acid, besides other well known salts; but after these salts are separated by lixiviation, there remains a *peculiar earth*, and a small proportion of iron. This earth differs from lime, baryt, magnesia, strontian, or any other known species of earth. I would call it *cinis*, for plentiful, common, and important as it is, science has not dignified it with a name. To judge of the excellence of this earth as a manure, after all the salts are extracted from soap boilers' ashes, the earth sells for ten cents the bushel, and notwithstanding this high price, it is not unusual for the farmer to pay for the article twelve months beforehand. When ploughed into sterile ground, at twelve loads per acre, it produces great crops of wheat, clover, and other sorts of grass and grain, and its fertilizing operation will last twenty years. Although some

* From Σκεδαω.

of

of the other ingredients of the ashes left after lixiviation may prove beneficial, yet the effects are chiefly from the cinis, or new named earth.

"This earth, which is so prized in America as a manure, was esteemed of old in Asia, as an ingredient in a cement: among the ancient Syrians, it was one of the materials forming the plaster of their walls; and as it holds an intermediate place between lime and potash, it can easily be conceived how it may act both as a cement and a manure. It is to be hoped, chemists will turn their attention to this important subject." See Tilloch's Philos. Magazine, Vol. VII. p. 273, for the whole of this interesting letter.

3. *Several Metallic Salts promote Vegetation, shewn by the Experiments of Professor Barton, of Philadelphia.*

Letter from Benjamin Smith Barton, M. D. Professor of Medicine in the College of Philadelphia, to Dr. Pearson, containing Experiments with Metallic Solutions to determine their Effects on Plants.

SIR,

Philadelphia, Oct. 28, 1802.

IN the *Annals of Medicine* for the year 1801, you inform us, that you have lately read a paper at the Board of Agriculture, "containing an account of the effects of a saline body collected from peat, as a most powerful manure, which turns out to be sulphate of iron; a substance (you remark) hitherto considered to be a poison to plants." This piece of intelligence gave me much satisfaction. I have, for some years, been engaged in an extensive series of experiments, relative to the effects of various stimulating articles, such as camphire, &c. upon vegetables; and on the absorption of certain powerful mineral substances into the organic system of vegetables. In numerous instances I have subjected the stems and leaves of plants, young and old, large and small, to the influence of the sulphates of iron and copper. I have found, that both of these metallic salts are very *greedily* absorbed by vegetables, insomuch that I have detected the presence of iron in the vessels of a branch of mulberry, at the height of five or six feet above the place of immersion in a solution of the sulphate of this metal. A full account of my experiments, I design to

Direct experiments on the effect of metallic salts on vegetation.

to communicate to the public in two memoirs. Permit me to obferve, in the meanwhile, that the fulphate of iron, applied to vegetables in the manner I have mentioned, "is only (to ufe your own words) a poifon, like almoft every thing elfe, from the over-dofe." *In feveral of my experiments, the branches of vegetables that were placed in veffels containing folutions of fulphate of iron and copper, lived longer and exhibited more figns of vigour, than fimilar branches that were placed in equal quantities of fimple water.* It is true, that, in many other experiments, thefe metallic falts proved fatal to my plants; but this was when I employed too large a dofe. In like manner I had found feveral years ago *, that camphire, by greatly ftimulating, often kills vegetables; and yet, when properly dofed, this is a very wholefome ftimulant to plants. I had alfo found, that large dofes of nitre (which is unqueftionably a powerful ftimulant both with refpect to animals and vegetables) produce an appearance like genuine gangrene in the leaves of vegetables: and yet it is certain that nitre, when it is judicioufly dofed, may be made to greatly affift the healthy vegetation of plants.

Excufe the liberty I have taken in troubling you with thefe few loofe hints, and permit me to fubfcribe myfelf,

Sir, your very humble and obedient Servant, &c.

BENJAMIN SMITH BARTON.

4. *Sulphate of Iron in the Peat of Ruffia, found by Profeffor Robifon.*

Sulphate of iron exifts in Ruffian peat.

Something elfe befides vegetable matter is neceffary to form peat or black mofs of the moors. The fmell of burning peat is different from that of vegetable matter. Peat afhes, fays the Profeffor, always contain a very great proportion of iron; he has feen three places in Ruffia where there is fuperficial peat mofs, and in all of them *the vitriol is fo abundant as to effloreſce.* In particular, on a moor near St. Peterfburgh, the clods fhew the vitriol (fulphate of iron) every morning when the dew has evaporated. According to this learned Profeffor's obfervation, the fulphate of iron in pit coal may be accounted for in the following manner: "peat moffes form very regular

* See Tranfactions of the American Philofophical Society, Vol. IV. No. xxvii.

ftrata,

ſtrata, lying indeed on the ſurface; but if any operation of nature ſhould cover this with a deep load of other matter, it would be compreſſed and rendered very ſolid: and remaining for ages in that ſituation, might ripen into a ſubſtance very like pit coal. See the Medical and Chirurgical Review for November 1803.

5. Mr. Anſtey's Teſtimony of the Uſe of Peat Duſt and Peat Aſhes.

SIR,

Houghton Regis, Dec. 3, 1801.

I received yours, dated the 18th of November laſt, in which you requeſted me to inform you what experiment I had made from the turf duſt, taken from Tingrith Moor. I have made uſe of the aſhes and duſt near thirty years, and I frequently lay on from eighty to a hundred buſhels per acre. Our land is dry and very thin ſtapled, owing to the chalk rock laying ſo very near the ſurface; it encourages vegetation in moiſt warm weather; but when hot and dry, the reverſe. We never mix any other manure with it. It coſts about four-pence per buſhel, including all expences. Teſtimony of Mr. Anſtey.

We chiefly ſpread it on our ſeed graſs, clover, &c.

I am, Sir, your humble Servant,

JOS. ANSTEY.

IV.

On Spectacles. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

DEAR SIR,

Lynn, February 16, 1805.

IN a former paper, I pointed out a property in ſpectacles, which had been overlooked by the writers on optics; but as ſome of your readers entertain opinions contrary to mine, the ſubject ſeems to require further inveſtigation. Poſition reſpecting ſpectacles,

What I have advanced concerning ſpectacles is, that we ſee better by oblique, than by direct pencils of rays *. viz. that we ſee better by oblique, than by direct rays.

* Philoſophical Journal, Vol. VII. p. 291.

It

It has been demonftrated by mathematicians, that an oblique pencil of rays has its focus a little nearer the lens, than a direct pencil; but how much this difference amounts to, at different angles, feems not to have attracted their attention. This difference, however, is too confiderable to be difregarded, when the rays of light fall very obliquely.

Confiderations relating to oblique pencils.

For let L L (*Fig.* 3. *Pl.* xi.) reprefent a double convex lens, F its focus, A F its axis, and *a b* a ray parallel to it, which will be refracted to F, after paffing through the lens. Alfo let *c b* and *d d* be two oblique rays falling upon the lens, at *b* and *d*.

Then the rays near A F falling almoft perpendicularly upon the fides of the lens, are lefs bent than the rays *c b, d d,* which fall obliquely on both fides of the lens. "For the more oblique they are, the more they are bent, and turned out of their direct way. And confequently the oblique rays *c b, d d,* will fooner interfect in G, than thofe at F." (Emerfon's Optics, page 124.) When the angle *a b c* is very confiderable, the difference between *x* F, and *y* G, will likewife be very confiderable. And the following fhort account of my experiments will throw fome light upon the truth of this propofition.

Spectacles obliquely placed.

In the fpectacles that I ufe, the reading diftance of oblique rays is four inches nearer the glaffes, than the diftance at which I can fee beft, with the direct rays.

And by experiments made with a lens of 24 inches focal diftance, and 4 inches in diameter, contracted to $\frac{3}{4}$ of an inch, I found the focal diftance of an oblique pencil of rays 8 inches fhorter than the focal diftance of the direct rays. The axis of the lens, when placed for the oblique rays, made an angle of 26° with a line drawn from its centre, to the candle with which the experiments were made. And the diftance between the candle and the lens meafured about 10 feet, when the lens was placed to receive the direct rays.

Cor. 1. When an object is feen through fpectacles, by oblique rays, it appears larger than by direct rays.

For the object is feen under the greateft angle, by thofe rays that are moft refracted.

Cor. 2. When an object is viewed by oblique rays through fpectacles, it is feen more diftinctly with one eye, than with the other.

For

For the rays fall more obliquely upon one of the glaffes, than upon the other.

Cor. 3. The direct pencils of rays are but feldom ufed, by a perfon whofe fpectacles are of the longeft focal diftance, that will afford him diftinct vifion.

For he generally looks through thofe parts of his glaffes, which are remote from their centres, in confequence of that motion which is given to the eyes, to view objects in different directions.

I am, Dear Sir,

Your moft obedient Servant,

EZEKIEL WALKER.

V.

Remarks on certain Paffages in Dr. Thomfon's Chemiftry, together with fome Experiments on Sandarach and Maftic. In a Letter from Mr. R. MATTHEWS.

To Mr. NICHOLSON.

SIR,

IN a work of fuch importance as Dr. Thomfon's Chemiftry, which muft be fo frequently referred to by the lovers of that fcience, the flighteft inaccuracy ought not to pafs unnoticed: the following obfervations therefore, perhaps may be confidered fufficiently interefting to deferve a place in your valuable Journal.

In Dr. Thomfon's chemiftry,

In his 4th Vol. page 307, Dr. Thomfon has the following remark: "Hitherto it has been affirmed by all chemifts both ancient and modern, that the alkalies do not exert any action on the refins." This remark I admit may be generally true; but although moft chemifts have been negligent in afcertaining the effects of alcalies upon fubftances of this nature, I conceive fome of them were not unacquainted with the power they poffefs of diffolving refinous bodies.

it is ftated, that Chemifts formerly afferted, that alcalies do not act upon refins.

Boerhaave, for inftance, in his elements of chemiftry, page 545, exprefsly fays, that, "both the fixed and volatile alcalies have a diffolving power, upon animal, vegetable, and mineral fubftances, fo far as thefe contain oils, balfams, gums, *rofins* or gummy rofins, &c."

The contrary is afferted by Boerhaave;

 Hoffmann

and by Hoffmann. Hoffmann alſo, by the method he employed in diſſolving amber with cauſtic nitre, evinced that he was acquainted with this property in alcalies. Dr. Thomſon deſcribes Mr. Hatchett as the firſt diſcoverer of the action of alcalies upon reſin. The facts above quoted will leſſen the originality, without diminiſhing the value of Mr. Hatchett's ingenious experiments.

How therefore it can be true that, "all chemiſts both ancient and modern, have affirmed, that the alcalies do not exert any action on the reſins" I cannot eaſily diſcern, unleſs indeed we impute to them the moſt palpable contradictions, an error which I believe is not often found in their writings.

Dr. Thomſon aſſerts, that one fifth of ſandarach is inſoluble in alcohol. Page 340 in the ſame volume, Dr. Thomſon aſſerts, that ſandarach is not ſoluble in alcohol, about a fifth part of it remaining undiſſolved; and deſcribes the inſoluble part as poſſeſſing peculiar properties, differing from ſandarach, and calls it *ſandaracha*. It ought not to be concluded, that Dr. Thomſon himſelf can have chemically examined every ſubſtance he muſt have occaſion to deſcribe in a work which embraces ſo great a variety; but the importance of ſandarach as an ingredient in varniſhes being nearly equal to that of any other reſin, this ſubſtance certainly deſerves more attention from chemiſts than has hitherto been beſtowed on it. The properties of this reſin as mentioned by Dr. Thomſon, are ſtated to reſt ſolely on the authority of Gieſe, no other chemiſt having examined it.

Experiment. It proved ſoluble in eight times its weight of alcohol. Upon trial I found that ſandarach was ſoluble in eight times its weight of alcohol, a very minute proportion only being left, which appeared to conſiſt almoſt entirely of extraneous matter. Conſequently the ſubſtance Dr. Thomſon has denominated *ſandaracha* can have no exiſtence in this reſin. It is proper to obſerve, that the reſin I made uſe of was ſelected fine, and is known in commerce by the names of gum juniper and ſandarach. From the obvious diſagreement in the reſults, perhaps we ought to conclude, that the ſubſtance employed by Gieſe in his experiments was not true gum ſandarach *.

* Looking to ſome chemical notes, I find that when three or four tears of ſandarach were put into pure alcohol, and left for a day, a ſmall quantity of thick fluid remained at the bottom, which, however, was taken up by agitation, and the whole became ſemi-opake:—It is remarkable, that this ſubſtance is not ſoluble in tallow or oil, as common reſin is. N.

Speaking of maftich, vol. 3, p. 311. Dr. Thomfon fays, it is readily foluble in alcohol. In the fame valuable volume, page 332, he informs us, that "that part of maftich which is infoluble in alcohol is faid to be pure caoutchouc." In inveftigating this apparent contradiction, I examined the action of alcohol upon maftich, and found that nearly a fifth part of it remained undiffolved. This refiduum, after repeated wafhings in alcohol, was white, confiderably elaftic, and adhefive; it was inflammable; and when heated became brown, emitting inflammable gas. In this ftate it had much the appearance of common Indian rubber, but fomewhat glutinous: It was not in the leaft acted on by water.

Dr. Thomfon's account of maftich.

The Author's experiments.

Alcohol leaves one fifth of maftich undiffolved, refembling Indian rubber.

I was induced to try the effects of different menftrua on this refiduum, and on the elaftic gum caoutchouc, with a view to afcertain their identity or otherwife. The following are the refults I obtained:

Comparative experiments on the refidue of mastich, and on caoutchouc.

Sulphuric ether, previoufly wafhed with diftilled water, diffolved this refiduum as well as caoutchouc.

Alcohol precipitated both thefe fubftances from their folvent in the form of a white curd.

Water had no action on the folutions.

By the nitric acid the refiduum was converted into a yellow brittle porous mafs, nitrous gas being difengaged from the acid.

Nitric acid did not act fo eafily on caoutchouc; but when raifed to the boiling point it changed it into a fimilar fubftance.

With the fulphuric acid a fubftance like charcoal was formed, the acid affuming a dark port wine colour, part of it being converted into fulphureous acid.

The fame effect was produced by the fulphuric acid on caoutchouc, though not without the affiftance of heat.

Neither the muriatic or oxymuriatic acids appeared to have any action on either of thefe fubftances.

Acetic acid diffolved a fmall portion of the refiduum, but had no action on caoutchouc.

Solutions of potafh and ammonia produced no effect on either the refiduum or caoutchouc.

From the above experiments it will be obferved, that the acids act with greater facility on the infoluble part of maftich, than on caoutchouc. Notwithftanding this difference in their action,

Thefe appear to be the fame.

action, I think it may be inferred, from the coincidence in their effects, that these bodies are similar substances. Probably the action of the acids on the residuum was assisted by a small portion of resin which it was combined with, and which it defended from the action of the alcohol employed to dissolve the mastich.

I am, your obedient Servant,

R. MATTHEWS.

March 13, 1805.

VI.

The Dutch Method of curing Herrings, extracted and translated from the German of Krünitz's Economical Encyclopædia (Oeconomische Encyclopädie), *Article Häring, by* J. HINCKLEY, *Esq. F. S. A.*

(Concluded from p. 224.)

Dutch method of catching and curing herrings.

HERRINGS, however, are equally good, wherever they are taken, provided they be but caught in the proper season, and well managed. As they die immediately on quitting their element, salting and packing are the circumstances which principally affect their quality. The superior excellence and flavour of Dutch herrings, above those of all other countries, arises from the close attention and indefatigable industry employed. Every thing, however minute, both as to season and management, which can maintain the reputation they have enjoyed for more than two hundred years, is most punctually observed; and above thirty ordinances on the curing and management of herrings are recited by Sir William Temple, in the 17th century.

Herrings, cured with Scotch salt, very quickly decay. Those of Norway are cured in the same manner as the Scotch, but with French salt, and packed in fir or deal; in consequence of which they are worse, and less palatable, as they leave a sour taste in the mouth, and soon spoil. In like manner, other nations are equally careless in assorting the fish, and to this may be attributed the precedency which the Dutch herrings have so long maintained. The Dutch catch their fish regularly and early off Hitland, from the 25th of June, because

because they are then, and to the beginning of July, fatteſt; after which time, the nearer they approach the coaſt, the leaner and worſe they are. Dutch method of catching and curing herrings.

It redounds no leſs to the honour of the Dutch, than to their advantage, that they pay the utmoſt poſſible attention to theſe rules: according to which, the fiſh muſt be taken at the proper ſeaſon, properly ſalted, well aſſorted, and rightly packed; to do all which, the captain and ſailors are by ſeveral laws obliged to bind themſelves by oath, before they ſail. There are alſo overſeers well paid, that they may not betray their truſt, but watch and enforce every the minuteſt regulation; to which circumſtance alſo, the pre-eminence of Dutch herrings throughout the world may partly be aſcribed.

As ſoon as the herrings are taken out of the water, they are thrown either upon the end of the deck, which has been cleared, and made perfectly clean for the purpoſe, or into baſkets; and then *gipped* (the gills and guts taken out) with a knife, by ſome of the crew, who are ſolely employed therein, having been brought up to that practice. The milt, or roe, however, is always left in the fiſh.* What are taken during one night, are, before the following ſun-ſet, neatly and ſkilfully laid in oaken barrels, coarſe Spaniſh or Portugueſe bay ſalt being ſtrewed between. This the fiſhermen of other countries either entirely neglect, or leſs carefully perform, being leſs ſcrupulouſly nice; becauſe they either go out to ſea later, or, like the Scotch, commence fiſhing too ſoon; or only navigate ſmall boats near the coaſt; do not kill the fiſh with a knife, or gut, ſalt, or pack them, while on board, but throw them down in a boat, and when fully laden, go on ſhore, proceed at their leiſure, caſt the fiſh on the ſea-coaſt in conſiderable heaps, where they are even ſuffered to lie ſometimes ſeveral days, before they are gutted, ſalted, and packed, in conſequence of which they grow ſtale and ferment. The Dutch, on the contrary, indefatigably purſue their method day and night, during twenty, twenty-four, or twenty-ſix weeks, be the weather what it may. Hence, their fiſh are uſually more tender, better flavoured, and not ſo very ſalt as the Engliſh and the Scotch.

* Mr. M'Culloch's Treatiſe (ſee note, page 413) ſays, they ſhould alſo be kept cleanly, and out of the ſun, as well as froſt or rain.

There

Dutch method of catching and curing herrings.

There are two methods of ſalting and preſerving herrings for a conſiderable length of time. The one is called white ſalting, the other red. The former is thus performed. Immediately on being taken, the fiſh are gutted, as above deſcribed, and waſhed in clean water. Then ſalt is ſprinkled on them, either internally, or both within and without, and the fiſh, being thrown into large baſkets with handles, are well rouſed, (or ſhaken about a few times) that the ſalt may the better diffuſe itſelf, and penetrate: or, laſtly, which is the beſt method, they are thrown into a tub filled with a ſtrong brine made with bay ſalt and freſh water, in which an egg will ſwim. In this pickle they are left upon deck in the open air, provided the weather be good, during twelve or fifteen hours; but, if circumſtances require, a good deal longer, and are well ſtirred (eſpecially if pickled on ſhore) with ſhovels ſeveral times, that the ſalt may the better and more generally penetrate every where. Laſtly, to pack them properly, they are taken out of the pickle, ſuffered to drain ſufficiently, and then packed in barrels, which are ſtrewed at bottom pretty thickly with ſalt, and, if there is time enough, they are neatly laid in ſtrata or layers, always ſtrewing ſufficient ſalt upon each layer. But, if the abundance of fiſh be too great, they are thrown in promiſcuouſly, with as much ſalt as is requiſite to preſerve them from ſpoiling. When this is done in ſtrata, each new layer is preſſed down hard upon the preceding. This laſt-mentioned proceſs, however, cannot eaſily be attended to at ſea, eſpecially when the fiſhery is very abundant. Hence, if the fiſh are to be exported, or remain long unuſed, they are re-packed on ſhore, laid in freſh ſalt, and preſſed down hard; without which precaution herrings exported by ſea would ſpoil. The ſame practice is purſued in France, Hamburgh, and doubtleſs elſewhere. At Hamburgh, as in Holland, they are packed in the open air, ten packers and three overſeers being appointed for the purpoſe, and all ſworn. Beſides re-packing, the packers in the maritime towns have alſo to pick and aſſort the fiſh, according to their goodneſs, ſalt them anew, and put them in freſh pickle. It is alſo generally a part of the magiſtrates and trading companies oaths, not to ſuffer any bad fiſh to be delivered from the quay or cuſtom-houſe.

After

Dutch method of catching and curing herrings.

After packing, whether the goods are intended for exportation or home-consumption, whether pressed down hard or not, they are regularly coopered, that the pickle may not leak out, and the fish turn yellow or spoil, which takes place the moment they are deficient in pickle. Properly, the herrings should, on the very day on which they are taken, not only be gutted, but salted and put in casks, or at least should not lie more than one night in the first pickle. And accordingly such herrings are distinguished in France by the name of *Harengs d'une nuit*. But, when the fishery is abundant, this is not always possible; so that only a part can be properly attended to; and the rest, after being gutted, must unavoidably remain, at least the whole following day, if not longer, in the first pickle, the regular packing of them being postponed till the third day. These fish, having stood two nights on deck in the open air, are called *Harengs de deux nuits*. But such goods not only are inferior, but do not keep so well as the former.

If the fishermen mean again to cast their nets on the following night, or if, on account of the great abundance of the shoals, they do not expect to complete the salting and regular packing in two days, the fish, which they cannot so complete, are salted in large heaps, and are then called *Slabbers*, or *Slabbegut*, coarse goods. These are frequently too salt, because want of time prevents their being properly managed. They are put into the schuyts, which always accompany the herring-busses, and washed; after which they are smoked; though not so much as the *Bicklings* (Bücklinge) or red-herrings. The salted herrings, hitherto spoken of, are called *Böckel herringe*, or pickled herrings, or, in general, plainly *Herrings*; those properly salted and packed in layers, *Packed*, or *Barrel herrings;* and those half salted, and promiscuously packed in barrels, *Wrack herrings.*

The other mode of curing, called Red salting, is thus performed. When the fish are taken out of the above-described pickle, in which, however, they must remain longer than those intended for the common, or white-salting, and at least four-and-twenty hours; they are hung by the head in rows on wooden poles, in stoves constructed for the purpose, each of which generally contains 12,000 herrings. Being thus placed, a fire is made under them with vine-stalks, or any green

Dutch method of catching and curing herrings.

green faggot-wood, that affords much ſmoke and little flame.* Here they remain till dried, and properly ſmoked, which generally requires twenty-four hours. Thus they become Bicklings, or Red-herrings; when theſe are packed in barrels, they are called Barrelled Bicklings, and are much ſalted; but, if laid in ſtraw, they are called Straw Bicklings, and are ſomewhat leſs ſalted. The excellence of the Bicklings principally conſiſts in their being large, fat, tender, freſh, properly ſalted, pliable, ſoft, of the colour of gold, and not torn or mangled. In Holland, the beſt fiſh are choſen for this purpoſe; but in other places, the above-deſcribed Slabbers only are uſed, or other inferior herrings, deemed unfit for the uſual mode of ſalting. The beſt and fatteſt ſmoked Dutch herrings are called, in German, *Speckbucklinge*, or fat red-herrings; in low Saxon, *Flickhäringe;* and in Hamburgh, *Flükheeringe*. They are cut open along the back.†

* At Bremen, the place moſt celebrated for ſmoking fiſh, and where no ſecret is made, as in Holland, of the proceſs, they are hung in ovens of the ſize of a ſmall parlour, and ſtrict attention is paid not to uſe fir, or any wood, in which is any the leaſt turpentine or reſinous matter, which invariably gives a bad taſte to the fiſh.

† The act of parliament regulations, and many uſeful obſervations, may be ſeen in two ſmall tracts, 8vo, in poſſeſſion of the Society for the Encouragement of Arts, Manufactures, and Commerce, the one entitled, "Obſervations on the Herring-Fiſhery upon the North and Eaſt Coaſts of Scotland, &c.; with plain Rules, propoſed for curing, and for ſupplying the London Market with White Herrings: By Lewis M'Culloch, many Years employed in furniſhing the Merchants of London with Herrings for Exportation. London. Richardſon, 1788." The other entitled, "The beſt and moſt approved Method of curing White Herrings, and all Kinds of White Fiſh; containing particular Directions how to ſlit, gut, ſalt, dry, and barrel them, fit for Sale at home or foreign Markets; with Directions for boiling of Oil: By a Trader in Fiſh. London. J. Davidſon, 1750."

The

VII.

Th Denfity of Mercury in its folid State afcertained. By Mr. JOHN BIDDLE.

To Mr. NICHOLSON.

SIR, *Birmingham, March* 4, 1805.

AT the Philofophical Society in Birmingham, of which I am a member, I read a few weeks ago a lecture on mercury. In the courfe of my experiments on this fubject, conducted with a view of exhibiting this metal in a folid ftate, by expofure to the frigorific mixture of muriate of lime and fnow, it appeared to me the fpecific gravity of this metal would be moft accurately obtained in comparifon with other metals under fimilar circumftances of folidity and temperature. I therefore undertook, amongft a variety of others, the following experiments, to afcertain its fpecific gravity in a folid ftate, which I believe had never before been accomplifhed: They fucceeded to my fatisfaction, and to the gratification of fome who were prefent: I am, therefore, inclined to believe, you will think them worth inferting in your Philofophical Journal.

Experiments before the Philofophical Society in Birmingham.

I am very refpectfully yours,

JOHN BIDDLE.

HAVING purified mercury, by diftilling from an earthen retort into a glafs receiver 50 *per cent.* of the quantity put in, to avoid the alloy of any other metal, and afterwards having heated it to 300° of Fahrenheit, to drive off any water that might adhere, fuch as was ufed in other experiments, and expofed to the action of muriate of lime and fnow,—I proceeded as follows:

Mercury was purified by diftillation;

One thoufand grains of this mercury were introduced to the freezing-mixture, together with three ounces of alcohol, in a round-bottomed glafs; and having placed in the mercury a fine bent wire, the weight of which was previoufly known when immerfed in the fame alcohol, to a certain point, in the temperature of 47 degrees above zero of Fahrenheit: this wire, during the congelation of the mercury, was fixed in it, fo that

and expofed to congeal under alcohol.

that the whole became attached to the inner furface of the glafs, and could not be feparated until the glafs with its contents was taken out of the freezing-mixture, and warmed, by dipping it in water, fo as to foften the mercury at the furface attached to the glafs; one hand being applied to the wire at the fame time, to draw the mercury from the glafs at the moment of its being loofened: it was then replaced, as before, in the freezing-mixture.

Vifible figns of great contraction of the mercury by freezing.

During the congelation of the mercury it was obferved, the furface of the metal towards the centre was very confiderably depreffed by the contraction of its particles, and the veffel having been moved during its congelation, a fmall hole nearly reaching the bottom of the glafs, was obferved gradually leffening in its dimenfions, prefenting a conical cavity with the apex downwards. The wire which held the mercury attached to it was now fixed by the other bent end to the hydroftatic balance, which held in the oppofite fcale the weights by which the mercury had been weighed in the air, together with the balance of the wire immerfed in the fame alcohol to a point marked on the wire.

Lofs of weight by the immerfion.

It appeared by weighing the 1000 grains of mercury thus immerfed in alcohol, the lofs of weight was 59,8 grains: this was weighed five or fix times, with the glafs ftill in the freezing mixture, with the fame refult; but when it was withdrawn only a little way from the muriate of lime and fnow, the difference of weight was perceptible, in confequence of the temperature of the mercury and alcohol increafing.

Comparative experiment with an equal weight of filver.

One thoufand grains of pure filver, weighed by the fame fcale immerfed in the fame alcohol at the fame temperature, loft in weight 88,105 grains: therefore, as the lofs of the weight of the mercury is to the lofs of weight in the filver, fo is the fpecific gravity of the filver to the fpecific gravity of the mercury.

Whence the fpecific gravity of folid mercury is deduced = 15.612.

The fpecific gravity of the filver in the fame balance having been afcertained, by diftilled water, to be 10,436, it follows, if this fum be multiplied into the fum of its lofs fufpended and weighed in alcohol, and divided by the fum of lofs of mercury weighed in the fame way, that the fpecific gravity of the mercury in the folid ftate, at about 40 degrees below zero on Fahrenheit's fcale, equals 15,612.

By

By the fame hydroftatic balance it appears, the fame mercury in a fluid ftate, the thermometer ftanding at 47 degrees above zero, is 13,545. Sp. Gr. when fluid, was 13,545.

By thefe experiments it appears, the difference of denfity between its fluid ftate, at the temperature of 47° above zero, and its folid ftate, at 40° below zero, is 2,0673 in 13545, or 1,5265 in 10, which is 15,265 *per cent.* that is, nearly $\frac{1}{7}$th of its greateft volume, or nearly $\frac{1}{6}$th of its leaft volume.

VIII.

Obfervations on Mr. Boswell's *Geometrical Propofitions. By* An Old Correspondent.

To Mr. NICHOLSON.

SIR, *March* 6, 1805.

When your ingenious correfpondent Mr. Bofwell fent to you his difcovery of a ready method of determining a chord-line equal to the fide of a fquare of the fame area as the circle in which it is drawn, it fhould feem that he was not aware that the quantum of error might be afcertained by calculation, as you have done in the fubjoined note, otherwife he would not have fatisfied himfelf with the mechanical proof which he has adduced as a teft of its accuracy. Had it occurred to you, I am perfuaded you would with equal facility have fhewn, that this gentleman's other "fact in geometry" is ftill more erroneous, though announced with a degree of confidence which appears to have arifen from a miftaken conviction of fuch obvious accuracy as ftood not in need of proof. On Mr. Bofwell's lines drawn in a circle.

"*A right line* (B E), fays he, *drawn from the extremity* B *of the line* I B, *at right angles through the oppofite diameter* (I F) *to the circumference, will be equal to a fourth of the circumference.*"

Let us try how far this affertion is founded in truth. But before we enter upon the examination, I will beg leave to point out a circumftance in your note, page 152 in your laft number, which, having arifen out of inadvertence in your tranfcribing Overfight by inverfion of terms in W. N.'s note.

tranfcribing it moft probably, may tend to puzzle your readers, and among the reft perhaps Mr. Bofwell himfelf, who muft be the moft interefted: You have faid, "as the radius is to IW, fo is the diameter to IB," (Plate VIII. Fig. 10); but I take it for granted you meant, "*as* I W *is to radius, fo is the diameter to* I B," becaufe this analogy is agreeable to the refult of your calculation. And as you have not mentioned how the doctrine of fimilar triangles applies in the conftruction of the figure, I hope you and your numerous readers will not deem me prefumptuous, if I introduce the diagram again with fome additions, particularly as it will give me an opportunity of proving more intelligibly the error of the fecond propofition already quoted. Any triangle formed in a femicircle, of which the diameter is one fide, with its oppofite angle at the circumference, is a right angled triangle (Eucl. III. Prop. xxxi.): This will be the cafe in Mr. Bofwell's figure if a right line be drawn from B to F, the right angle being at B; and as the angle at I is common to the fmall right angled triangle I O W, and to the large one I B F, the two triangles are fimilar, and therefore the hypothenufe I W of the fmall one is to its bafe I O, as the hypothenufe I F (or diameter) is to the bafe I B of the large one. Q. E. D.

Farther elucidation of the note.

How far Mr. B.'s fecond propofition is accurate.

With refpect to the fecond propofition, it is very well known (Eucl. III. Prop. xx.) that the angle formed at the centre of a circle is double of the angle at the circumference, if they have the fame common chord as a bafe; now in the triangle I O W (Plate XI. Fig. 4), the fides I O being given $= .5$, O W $= .25$, and therefore I W $\doteq .55901$, we have, by a fimple cafe in plane trigonometry, the angle at I $= 26^\circ\ 33'\ 5''.4$, the double of which is $53^\circ\ 6'\ 10''.86$ for the angle B O F at the centre; but B G, one half of the line B E in queftion, is the fine of this angle, and in natural numbers is, by the tables, equal to .7997163, when unity is radius; but, in our figure, the diameter reprefents unity, therefore the double fine, or chord line, B E is $= .7997163$, which, according to Mr. Bofwell's affertion, ought to be $= .7854$, or one-fourth of the circumference 3.1416; hence the error is .0143163, or very nearly $\frac{1}{70}$th of the whole.

Apology to thefe remarks.

As I have not the pleafure of knowing Mr. Bofwell, notwithftanding I have admired feveral fpecimens of his ingenuity, and as I profefs to be a promoter of all arithmetical approximations

proximations and ſcientific projects, that have for their means facility of application, and utility for their object, I mean ſo far as they have accuracy to recommend them, I truſt he will not impute any motive of a perſonal nature to me, but give me credit for the declaration, when I aſſert, that the implied invitation collected from his memoir, to aſcertain the accuracy of any of his projects, has been my only inducement for laying before the public the preſent paper.

I am, Sir,

With due reſpect,

AN OLD CORRESPONDENT.

IX.

A Memoir on Milk and the Lactic Acid. By Cit. BOUILLON LAGRANGE.

(Concluded from page 144.)

THE muriate of ſoda, and more eſpecially the muriate of lime, affords a proof of this. Theſe ſalts effect a ſeparation of the curd only in part; and, nevertheleſs, the muriate of lime has a very great affinity for water: the decompoſition ought therefore, in this caſe, to be made in a more ſtriking manner than with the acids. Curd is not ſeparated by the mere abſtraction of water.

Whey is not therefore the product of fermentation. The alcohol obtained from milk does not prove that this formation is neceſſary in order to obtain the lactic acid; for this laſt may be obtained from a ſerum recently prepared. Whey not a product of fermentation.

With regard to the formation of the carbonic acid indicated in the ſixth experiment, it is owing to the decompoſition of a ſmall portion of animal matter, and part of the ſugar of milk diſſolved in the ſerum. The more of theſe ſubſtances is decompoſed the greater will be the quantity of carbonic acid formed, and accordingly the degree of acidity is more marked on account of the preſence of the carbonic acid; but if the acid be expelled, the ſerum is milder, and not ſo ſour as that which has been for ſome time expoſed to the air. Carbonic acid in whey.

If the ſerum be expoſed to the air for ten or twelve days, in proportion as the ſaccharine matter is decompoſed the whey becomes Acetic acid.

becomes more acid. Thefe degrees of acidity are owing to a fmall quantity of acetic acid, formed by the aid of carbonic acid and alcohol remaining in the liquor, which muft naturally augment the acidity of the acid already formed in the milk.

Whether the falts in whey render it four.

With regard to the fecond queftion, Whether whey exhibits acid properties only on account of the falts it holds in folution?

I have proved that the acid may be feparated from the faline matters without expofing the milk to the air for a certain time, as Scheele has directed; and the experiments I fhall relate in this memoir will afford additional proofs. So that its acid properties do not depend merely upon the falts it holds in folution, and the lactic acid is not produced by a fermentation fimilar to the acetous.

Zoonic acid by diftilling milk;

Though the action of caloric upon milk is generally known, I fhall neverthelefs make an obfervation upon the acid obtained by diftillation with a naked fire, which has for feveral years been known by the name of the Zoonic Acid.

is acetous acid.

If this acid be purified and combined with potafh, a falt is obtained, which prefents all the characters of the acetate of potafh.

Curd contains phofphate of lime.

I muft alfo obferve the cheefy matter in which fome chemifts have been unable to find phofphate of lime, as announced by Scheele. To obtain this, nothing more is neceffary than ftrongly to heat this fubftance in a crucible after having well wafhed it, and to calcine it to whitenefs.

The refidue is a hard white matter, of which fome fmall pieces, veined with blue, may be compared to the turquois.

The nitric acid diffolves it without effervefcence; lime-water and ammonia occafion a flocculent precipitate of phofphate of lime.

The oxalate of ammonia alfo demonftrates the prefence of lime.

SECTION III.

Concerning the Serum of Milk.

Examination of whey feparated by different agents.

Being defirous of knowing whether there is any difference between the wheys obtained from milk by different fubftances, I made ufe of the mineral acids, fome vegetable acids, falts, fuch as alum, the acidulous tartrite of potafh, &c. The coagulating fubftance was employed in due quantity; as I had

 remarked,

remarked, that when an excefs of acid is ufed, it is found to exift in the ferum; whereas, when the neceffary quantity is ufed, it is found in the cheefe only.

It is the fame fluid in the different cafes.

After having feparated the cheefy matter the ferum was clarified; the wheys were found to have all the fame tafte and colour, and poffeffed the fame degree of acidity.

It affects fyrup of violets.

This frefh whey turns fyrup of violets green: This property muft be attributed only to the union of the yellow and blue, and not to faline matters, as chemifts have afferted. Whey, which has loft its colour by expofure to the air, reddens the fyrup of violets.

The means employed to afcertain the acids which had been ufed to coagulate the milk, did not exhibit even the fmalleft quantity when the operation was performed with exactnefs.

Sour ferum by expofure of milk

I made a comparative examination of the ferum which had become four in the open air; and I obtained, by expofing the milk, the fame phenomena as were pointed out in the fecond fection and third experiment.

differed from other ferum.

The fpontaneous ferum differed, 1. In its tafte, which was more acid; and, 2. Its colour, which is dull and whitifh, owing to white filaments which are feparated. It may be had clear by filtration, without acquiring the colour of frefh whey.

Cheefy matter.

The cheefy matter refulting from the feveral experiments before related, gave the fame red with tincture of turnfole. If it be calcined and treated with the known re-agents, the prefence of fulphuric acid will be indicated if this acid has been employed in the coagulation, and alumine when alum has been made ufe of.

Alum or fulphuric acid may be employed to coagulate milk.

I do not therefore fee any inconvenience, in preparing whey for the purpofes of pharmacy, to coagulate the milk with one or the other of thefe fubftances, more efpecially if the precautions I have mentioned be attended to. Thofe who have thought it unwholefome to coagulate milk with alum, have doubtlefs made no experiment on the fubject.

Oxigen and ferum.

Oxigen gas is not abforbed by the ferum; even agitation does not facilitate its union.

Other agents.

The action of lime-water, the folution of barytes, ftrontian, and fome metallic falts, though already known by chemifts, require to be again examined, and will enable me to explain the proceffes for obtaining lactic acid.

SECTION IV.

On the Sugar of Milk.

Whether acetic acid in milk arise from the decomposition of its sugar.

The formation of acetic acid does not arise merely from the alteration of the sugar of milk; and though the experiments already described admit of certain conjectures, I have ascertained whether a like quantity of sugar of milk could be obtained from fresh serum and a serum exposed for twelve days at a temperature from 14 to 20 degrees.

A little sugar is decomposed by standing.

I have found that sour milk affords a somewhat less quantity of sugar of milk, which proves, that a portion of the saccharine matter was employed in forming a quantity of acetic acid.

We may therefore obtain carbonic acid, alcohol, and acetic acid, either by confining whey in close vessels, or by leaving it exposed to the air.

Other experiments made on pure sugar of milk indicated nothing remarkable.

Habitudes of sugar of milk.

It does not redden tincture of turnsole. When dissolved in water and exposed for a long time to the air, it does not become acid. By distillation it affords acetic acid along with the known products.

It is soluble in weak acetic acid. If a small quantity of fresh caseous matter be added, it is soon found that the liquor holds it in solution. By pouring in a few drops of alkali to saturate the excess of acid, it loses its transparency, becomes turbid and milky, with a mild taste resembling that of milk, and preserves its opacity for a considerable length of time.

General results respecting milk.

The following results are deducible from the preceding facts:—

1. New milk reddens the tincture of turnsole.

2. The cheesy matter may be separated without contact of the air.

3. The serum does not retain the acids made use of to coagulate milk.

4. Distillation either of milk or of sugar of milk to dryness, affords the acetic acid.

5. The formation of carbonic acid and alcohol is owing to muco-saccharine matter.

6. A quantity of acetic acid is formed by the fermentation of these substances.

7. The

7. The cheeſy matter, when ſeparated, always manifeſts the preſence of an acid, and differs in taſte and conſiſtence according to the matters employed in its ſeparation.

8. When waſhed and affording no ſign of acidity, if it be afterwards diffuſed in diſtilled water for ſeveral days, at a temperature between 15 and 22 degrees (about 65 and 80 Fahrenheit), it acquires a ſtrong diſagreeable odour; the fluid ſlightly reddens turnſole, and lime diſengages ammonia.

9. The ſerum and the cheeſy matter contain, beſides the known ſubſtances, phoſphate of lime, as Scheele had announced.

10. The difference which exiſts between freſh ſerum and that which has been expoſed to the air, conſiſts in the acetic acid, which is preſent along with the free acid in the milk.

11. Laſtly, there exiſts in milk and the ſerum a free or uncombined acid, which ſeemed to me to be the acetic acid.

SECTION V.

Concerning the Lactic Acid.

Lactic acid, not obtained by ſimple diſtillation.

Scheele having aſcertained that it is not poſſible to obtain the acid by ſimple diſtillation, had recourſe to peculiar methods to effect this ſeparation.

Scheele's proceſs. Evaporate whey to one-eighth.

This chemiſt firſt reduced the whey to one-eighth by evaporation; he then filtered it, and there remained, according to him, no more cheeſy matter.

Filter. Saturate with lime-water, which throws down phoſphate. Filter. Dilute with water. Precipitate lime by oxalic acid. Evaporate to mucous conſiſtence. Diſſolve the acid in alcohol and filter. Then add water, and diſtil off the alcohol. Reſidue is lactic acid.

He ſaturated the fluid with lime-water, and phoſphate of lime was precipitated: This liquor having been filtrated and diluted with three times its quantity of water, he poured in oxalic acid drop by drop, to ſeize and precipitate all the lime; and he aſcertained, by the addition of a little lime-water, that no more oxalic acid remained. He evaporated the liquor to the conſiſtence of honey; the thickened acid was rediſſolved in rectified alcohol; the ſugar of milk and all the foreign ſubſtances remained on the filtre, not having been diſſolved by the alcohol. Laſtly, after having again added a little water to the acid held in ſolution by the alcohol, he drew off this laſt fluid by diſtillation, and found the lactic acid in the retort.

It is not pure.

This long and expenſive proceſs does not afford a pure acid. It was therefore neceſſary to adopt a ſimpler proceſs before its nature was examined.

Experiment

With fresh whey little acid is obtained.

Experiment 1. If instead of exposing whey to the air as Scheele directs, the process be immediately performed on the fresh whey, a very small quantity only of acid is obtained, which is less coloured and has a more animal smell.

This acid appeared to me to be uncombined in the milk, as its quantity was so small.

If barytes or strontian and fulphuric acid be used instead of the lime-water and oxalic acid of Scheele the result is equally perfect.

Experiment 2. Instead of using lime-water and the oxalic acid, I used barytes or strontian and the fulphuric acid. By this means I obtained an acid no less pure than that of the Swedish chemist.

Process with acetate of lead unsuccessful.

Experiment 3. I had likewise attempted to decompose a lactate of potash with acetate of lead, expecting to form a lactate of lead decomposable by the fulphuric acid; but the precipitate thus formed is not a lactate of lead, for when a sufficient quantity of fulphuric acid is added for the purpose of decomposing it, no uncombined acid is set at liberty. It appears that part of the oxide falls down with the animal matter; and this compound is even to a certain degree soluble. It is also found that the fluid always retains much acetate of lead in solution.

We therefore see that though this process has been pointed out as superior to that of Scheele, it is certain that it cannot be employed to obtain the lactic acid. The following experiments were made on the lactic acid of Scheele.

Lactic acid of Scheele examined.

1. The intensity of its colour may be diminished by grinding a piece of red hot charcoal in a mortar, and pouring the lactic acid upon it. A quantity of small flakes were separated which rose to the surface. The fluid was slightly boiled and filtered, and the acid was then less coloured, and had less odour.

2. When this acid is distilled in a retort acetic acid passes over and a thick yellow very acid matter remains.

3. It may perhaps be thought that part of this acid becomes changed into acetic acid by the assistance of caloric, as Berthollet observes in his Chemical Statics.

"The ternary acids, as this chemist observes, may be changed into acetic acid, when by the action of heat they abandon part of their carbon, and their elements, which resist this action less, are separated by volatilazation."

I have ascertained that great part of this acid previously existed in the fluid.

3. The

3. The ſame phenomena take place, but more ſtrikingly, if whey be diſtilled which has been expoſed to the air at the temperature of 15 or 20 degrees (60 and 68 Fahrenheit) for about a fortnight. The firſt product is a clear tranſparent fluid of an acid taſte, and odour of alcohol, which when combined with potaſh, forms an acetate.

If alcohol be added to what remained in the retort, it becomes coloured. After decantation and diſtillation, an acid is found in the retort of a peculiar odour, animalized, and reſembling the preceding; but the acid contains a very ſmall quantity of phoſphate of lime.

Theſe experiments confirm the obſervations I have made: 1. That a mixed acid is obtained by the proceſs of Scheele, and 2. That an acid exiſts in milk and in whey recently prepared. It remains to ſhew the nature of that acid. For that purpoſe I prepared the lactic acid according to the proceſs of Scheele with every neceſſary precaution. I even aſcertained the purity of my alcohol, which having been diſtilled from muriate of lime, marked 40 degrees of the aerometer of Reaumur.

It is a mixed acid.

The lactic acid when diſſolved in alcohol was ſubmitted to diſtillation. There paſſed into the receiver an alcohol which reddened the tincture of turnſole.

When diſſolved in alcohol ſome acid comes over with the ſpirit.

When the acid was totally deprived of alcohol the diſtillation was ſtopped. The lactic acid of Scheele remained of a yellow colour and very ſour taſte.

Lactic acid remained.

Experiment 1. I mixed quick lime with this concentrated acid, and a diſengagement of ammonia took place.*

It gave out ammonia by lime.

Experiment 2. If a lactate of potaſh be formed and evaporated by a gentle heat, a brown matter is ſeparated, which is inſoluble in water. This being afterwards heated in a crucible, the ſalt ſwells up and emits an animal odour. The preſence of pruſſic acid was proved by diſſolving the remaining matter in water and adding a ſmall portion of the ſulphate of iron in ſolution.

Lactate of potaſh leaves pruſſic acid by heat.

* I had before aſcertained the preſence of ammonia in this acid. I announced it on the 15th Nivoſe to the Society of Pharmacy. Mr. Vauquelin, who was not acquainted with this fact, informed me at the reading of the firſt part of this memoir that he had alſo aſcertained the preſence of an animal matter in the lactic acid of Scheele.

Experiment

Sulph. acid added to lactic acid disengages acetous acid.

Experiment 3. If sulphuric acid be added to lactic acid, or to a lactate of potash, a disengagement takes place not only of acetic acid, but also of an elastic fluid, which forms a thick cloud when in contact with ammonia.

Distillation.

Experiment 4. When the preceding mixture is distilled in an apparatus proper to receive the gaseous product in water, the distilled water of the bottle preserves its transparency; the smell of acetic acid is very evident; it reddens turnsole and precipitates the muriate of silver.

When a small quantity of ammonia was brought near the tube out of which the gas issued, a very dense cloud was formed.

Fire decomposed the acid of the lactate of potash.

Experiment 5. What was left in the retort was evaporated to dryness. This residue was brown, very acid, and when afterwards heated in a crucible of platina, swelled up and left a coal.

During this operation a small quantity of sulphuric acid was decomposed.

The matter that remained in the retort was dissolved in distilled water. The solution did not redden turnsole, but it gave a brown colour to paper tinged with curcuma. A little sulphuric acid was added to saturation, and after evaporation crystals were obtained by cooling, which exhibited the characters of sulphate of potash.

As the supernatant liquor had a metallic taste, prussiate of lime was poured on it, which produced a bluish tinge.

Lactic acid decomposed by fire leaves potash.

Experiment 6. The lactic acid may also be concentrated to dryness; heated to incineration in a crucible; afterwards dissolved in a distilled water and precipitated by nitrate of silver. A muriate of silver is formed which may be separated by the filter; and the fluid which contains the nitrate is then evaporated and decomposed by heat. A white matter remains, which when dissolved in water, affords with the tartareous acid an acidulous tartrite of potash.

Component parts of Scheele's lactic acid.

Hence I conclude that the lactic acid of Scheele is composed of acetic acid; muriate of potash; a small portion of iron probably dissolved in the acetic acid; and an animal matter.

An

X.

An Analyſis of the Magnetical Pyrites; with Remarks on ſome of the other Sulphurets of Iron. By CHARLES HATCHETT, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

§ I.

OF the various metallic ſulphurets which conſtitute one of the grand diviſions of ores, none appear to be ſo univerſally diſperſed throughout the globe, as the ſulphuret of iron, commonly called Martial Pyrites; for the ſpecies and varieties of this are found at all depths, and in all climates and ſoils, whether ancient, or of alluvial and recent formation. It is remarkable alſo, that, under certain circumſtances, this ſulphuret is daily produced in the humid way; an inſtance of which, a few years back, I had the honour, in conjunction with Mr. Wiſeman, to lay before this Society; * and although, in regard to pecuniary value, the pyrites of iron may be conſidered as comparatively inſignificant, yet there is every reaſon to believe, that in the operations of nature, it is a ſubſtance of very conſiderable importance.

Univerſal diffuſion and importance of ſulphurets of iron.

§ II.

The ſpecies and varieties of martial pyrites, are in general ſo well known, and have been ſo frequently and accurately deſcribed, as to figure, luſtre, colour, and other external characters, that it would be totally ſuperfluous here to give any detailed account of them. One of the ſpecies, however, merits peculiar notice, as poſſeſſing the remarkable property of ſtrong magnetic polarity; and, although it has been deſcribed by modern mineralogiſts,† it does not appear to have been as yet ſubjected to any regular chemical examination; ſo that, whether it be a ſulphuret of iron inherently endowed with the magnetical property, or a ſulphuret in which particles of the ordinary magnetical iron ore are ſimply but minutely interſperſed, has to this time remained undecided.

Among the numerous ſpecies one is remarkable for ſtrong magnetic polarity.

* Phil. Tranſ. for 1798, p. 567.

† Kirwan, Vol. II. p. 79. Widenmann, p. 792. Emmerling, 2d edit. Tom. II. p. 286. Karſten, p. 48. Brochant, Tome II. p. 232.

This

Its obvious or external properties.

This ſpecies is known by the name of Magnetical Pyrites, and is called by the Germans *Magnet-Kies*, or *Ferrum mineraliſatum magnetico-pyritaceum.*

It is moſt frequently of the colour of bronze, paſſing to a pale cupreous-red.

The luſtre is metallic.

The fracture is unequal, and commonly coarſe-grained, but ſometimes imperfectly conchoidal.

The fragments are amorphous.

The trace is yellowiſh-gray, with ſome metallic luſtre.

It is not very hard; but, when ſtruck with ſteel, ſparks are produced, although with ſome difficulty.

It is brittle, and is eaſily broken.

Where found.

This pyrites has been hitherto found only in ſome parts of Norway, Sileſia, Bavaria, and eſpecially at Geier, Meſſerſdorf, and Breitenbrunn in Saxony; but, having received ſome ſpecimens from the Right Hon. Charles Greville, F. R. S. I was ſtruck with their reſemblance to the pyrites of Breitenbrunn, which happened at that time to be in my poſſeſſion; and, upon trial, I found that they were magnetical, and agreed with the latter in every particular. Their magnetic power was ſuch as ſtrongly to affect a well-poized needle, of about three inches in length; a piece of the pyrites, nearly two inches ſquare, acted upon the needle at the diſtance of four inches.

Degrees of magnetiſm.

The powder (which is blackiſh-gray, with but little metallic luſtre) is immediately taken up by a common magnet; but the pyrites does not act thus on the powder, nor on iron filings, unleſs it has been placed for ſome time between magnetical bars; then indeed it acts powerfully, turns the needle completely round, attracts and takes up iron filings, and ſeems permanently to retain this addition to its original power.

In the ſpecimens which I obtained, the north pole was generally the ſtrongeſt.

This pyrites was found in Wales, about the year 1798, by the Hon. Robert Greville, F. R. S. who ſent the ſpecimens above deſcribed to his brother, the Right Hon. C. Greville, with the following account.

Abundantly found in Caernarvonſhire.

" It is found in great abundance in Caernarvonſhire, near " the baſe of the mountain called Moel Elion, or probably with " more accuracy Moel Ælia, and oppoſite to the mountain " called

"called Mynydd Mawr. Thefe mountains form the entrance "into a little clofe valley, which leads to Cywellin lake, near "Snowdon, a little beyond the hamlet of Bettws.

"The vein appears to be fome yards in depth and breadth, "and feems to run from north to fouth, as it is found on "Mynydd Mawr, which is acrofs the narrow valley, and "oppofite to Moel Ælia."

Mr. R. Greville, in another part of his letter, ftates that copper ore has been worked in feveral of the adjacent places, and that, many years ago, Capt. Williams, of Glan yr Avon, employed fome miners at the place where this pyrites is found, but the undertaking proved unproductive. Yellow copper ore is certainly in the vicinity; for fome portions of it were adhering to the fpecimens which have been mentioned; and I fhall here obferve, that the ftone which accompanies the magnetical pyrites, is a variety of the lapis ollaris or pot-ftone, of a pale grayifh-green, containing fmooth cubic cryftals of common pyrites.

§ III.

From the appearance of thofe parts of the magnetical pyrites which have been expofed to the weather, it feems to be liable to oxidizement, but not to vitriolization.

Magnetic py-rites is flightly oxidable by the weather.

The fpecific gravity, at temperature 65° of Fahrenheit, is 4518.

Sp. gravity.

When expofed to the blowpipe, it emits a fulphureous odour, and melts into a globule nearly black, which is attracted by the magnet.

Effect of mere heat by blow-pipe:

Five hundred grains, in coarfe powder, were expofed, in a fmall earthen retort, to a red heat, during three hours. By this operation, the weight of the powder was very little diminifhed; neither was there any appearance of fulphur in the receiver, which however fmelt ftrongly of fulphureous acid.

— in a crucible;

Five hundred grains of the fame were put into a flat porcelain crucible, which was kept in a red heat, under a muffle, during four hours. The powder then appeared of a dark gray, with a tinge of deep red, and weighed 432.50 grains. The lofs was therefore 67.50 = 13.50 *per cent.* but, upon examining the refiduum, I found that only part of the fulphur had been thus feparated.

— in the muffle.

The

Partly foluble in dilute fulph. acid.

The magnetical pyrites, when digefted in dilute fulphuric acid, is partially diffolved, with little effervefcence, although there is a very perceptible odour of fulphuretted hydrogen.

The folution is of a very pale green colour.

Precipitates.

Pure ammonia produced a dark green precipitate, tending to black; and pruffiate of potafh formed a very pale blue precipitate, or rather a white precipitate mingled with a fmall portion of blue. The whole of the latter, however, by expofure to the air, gradually affumed the ufual intenfity of Pruffian blue; and the blackifh green precipitate, formed by ammonia, became gradually ochraceous. Thefe effects therefore fully prove, that the iron in the folution was, for the greater part, at the minimum of oxidizement, fo as to form the green fulphate, and white pruffiate, of iron; * and, confequently, that the iron of the magnetical pyrites is either quite, or very nearly, in the ftate of perfect metal.

Effect of nitric acid.

This pyrites, when treated with nitric acid, of the fpecific gravity of 1.38, diluted with an equal quantity of water, is at firft but little affected; but, when heat is applied, it is diffolved, with much effervefcence, and difcharge of nitrous gas; the effervefcence, however, is by no means fo violent as when the common pyrites are treated in a fimilar manner. It is alfo worthy of notice, that if the digeftion be not of too long duration, a confiderable quantity of fulphur, *in fubftance,* is feparated; whilft, on the contrary, fcarcely any can be obtained from the common pyrites, when treated in a fimilar manner; although I fhall foon have occafion to prove, that the real quantity of fulphur is much more confiderable in the latter than in the former.

Of muriatic acid.

As foon as muriatic acid is poured on the powder of the magnetical pyrites, a flight effervefcence is produced, which becomes violently increafed by the application of heat; a quantity of gas is difcharged, which, by its odour, by its inflammability, by the colour of the flame, by the depofition of fulphur when burned, and by other properties, was proved to be fulphuretted hydrogen.

During the digeftion, fulphur was depofited, which fo enveloped a fmall part of the pyrites, as to protect it from the farther action of the acid.

* Récherches fur le Bleu de Pruffe, par M. Prouft. Annales de Chimie, Tome XXIII. p. 85.

The

The folution was of a pale yellowifh-green colour. With pruffiate of potafh it afforded a pale blue precipitate, or rather a white precipitate mixed with blue; and with ammonia it formed a dark blackifh-green precipitate, which gradually became ochraceous; fo that thefe effects corroborated the conclufions which were founded on the properties of the fulphuric folution, namely, that the iron contained in the pyrites, is almoft, if not quite, in the metallic ftate.

Muriatic folution.

Other experiments were made; but, as they merely confirm the above obfervations, I fhall proceed to give an account of the analyfis.

§ IV.

ANALYSIS OF THE MAGNETICAL PYRITES.

A. One hundred grains, reduced to a fine powder, were digefted with two ounces of muriatic acid, in a glafs matrafs placed in a fand bath. The effects already defcribed took place; and a pale yellowifh-green folution was formed. The refiduum was then again digefted with two parts of muriatic acid mixed with one of nitric acid; and a quantity of pure fulphur was obtained, which, being dried, weighed 14 grains.

Analyfis. 100 gr. magnetic pyrites were digefted in muriatic acid. Partial folution. Refidue treated with nitro-mur. acid. Some fulphur feparated.

B. The acid in which the refiduum had been digefted, was added to the firft muriatic folution; fome nitric acid was alfo poured in, to promote the oxidizement of the iron, and thereby to facilitate the precipitation of it by ammonia, which was added after the liquor had been boiled for a confiderable time. The precipitate thus obtained was boiled with lixivium of potafh; it was then edulcorated, dried, made red-hot with wax in a covered porcelain crucible, was completely taken up by a magnet, and, being weighed, amounted to 80 grains.

To the acid folutions was added fome nitric acid and the iron was then precip. by ammonia. The precip. was boiled with potafh and then reduced to black oxide.

C. The lixivium of potafh was examined by muriate of ammonia, but no alumina was obtained.

The potafh lixivium gave no alumina.

D. To the filtrated liquor from which the iron had been precipitated by ammonia, muriate of barytes was added, until it ceafed to produce any precipitate; this was then digefted with fome very dilute muriatic acid, was collected, wafhed, and, after expofure to a low red heat for a few minutes in a crucible of platina, weighed 155 grains. If therefore the quantity of fulphur, converted into fulphuric acid by the preceding operations, and precipitated by barytes, be calculated according to the accurate experiments of Mr. Chenevix, thefe 155 grains of fulphate

The firft folution liquor was precip. by muriate of barytes. The fulphate of barytes thus obtained fhewed the quantity of fulph. acid.

ſulphate of barytes will denote, nearly, 22.50 of ſulphur; ſo that, with the addition of the 14 grains previouſly obtained in ſubſtance, the total quantity will amount to 36.50.

Eſtimate of the pure iron in the oxide.

E. Moreover, from what has been ſtated it appears, that the iron which was obtained in the form of black oxide, weighed 80 grains; and, by adding theſe 80 grains to the 36.50 of ſulphur, an increaſe of weight is found = 16.50. This was evidently owing to the oxidizement of the iron, which, in the magnetical pyrites, exiſts quite, or very nearly, in the metallic ſtate, but, by the operations of the analyſis, had received this addition. The real quantity of iron muſt, on this account, be eſtimated at 63.50.

Component parts. Sulphur 4. and iron 7.

One hundred grains, therefore, of the magnetical pyrites, yielded,

$$\begin{array}{lccl} \text{Sulphur} & \left\{\begin{array}{l}\text{A.}\ \ 14\\ \text{D.}\ \ 22.50\end{array}\right. & \left.\right\} & 36.50 \text{ grains.} \\ \text{Iron} & \text{E.}\ = & & 63.50 \\ \hline & & & 100. \end{array}$$

Analyſis repeated by acidifying all the ſulphur by nitric acid. Same reſult.

This analyſis was repeated in a ſimilar manner, excepting that the whole was digeſted in nitric acid, until the ſulphur was intirely converted into ſulphuric acid. To the liquor which remained after the ſeparation of the iron by ammonia, muriate of barytes was added, as before, and formed a precipitate which weighed 245 grains. Now, as the ſulphuric acid in ſulphate of barytes is eſtimated by Mr. Chenevix at 23.5 *per cent.* and the ſulphur which is required to form the ſulphuric acid contained in 100 parts of ſulphate of barytes, at 14.5 *, it follows, that 245 grains of dry ſulphate of barytes, contain ſulphuric acid equal, very nearly, to 36 grains of ſulphur; ſo that the two analyſes corroborate each other. The proportion of ſulphur in the magnetical pyrites, may therefore be ſtated at 36.50, or indeed at 37 *per cent.* if ſome ſmall allowance be made for the occaſional preſence of earthy particles; a minute portion of quartz having been found, by the laſt analyſis, after the complete acidification of ſulphur.

The iron in this and in all pyrites, is very nearly in the metallic ſtate.

The increaſe produced, by the operations of the analyſis, in the weight of the iron, aroſe, as I have already remarked, from the addition of oxygen; for the iron, as obtained by the analyſis, was in the ſtate of black oxide; but in this, and

* Tranſactions of the Royal Iriſh Academy, Vol. VIII. p. 240.

indeed

indeed in all pyrites, it undoubtedly exifts very nearly, or quite, in the ftate of perfect metal. Now the black oxide of iron, called Protoxide by Dr. Thomfon *, has been proved, by Lavoifier and Prouft, to confift of 100 parts of metallic iron combined with 37 of oxygen, thus forming 137 of black oxide; the exact proportion of oxygen is therefore 27 *per cent*, and 80 grains of this oxide muft contain 21.6 of oxygen. But, in the above analyfes of the magnetical pyrites, the increafe of weight did not amount to more than 16.5; and we may therefore conclude that, in all probability, a quantity of oxygen = 5.1 was previoufly combined with fome part, or with the general mafs, of the iron in the pyrites. A fmall part of the abovementioned increafe of weight, muft likewife have arifen from another caufe; for, although the true proportions of the black oxide of iron are 27 of oxygen and 73 of iron, (fo that 100 parts of the latter abforb 37 of the former,) yet, in actual practice, it is difficult to obtain it exactly in this ftate, and there is commonly a fmall excefs of weight: this I have repeatedly obferved, in many experiments, fome of which were purpofely made. When, for inftance, 100 parts of fine iron wire were diffolved in muriatic acid, and afterwards precipitated by ammonia, edulcorated, dried, and made red-hot with a fmall quantity of wax in a covered porcelain crucible, the weight, inftead of 137, ufually amounted to 139 or 140. The quantity of wax employed, certainly did not afford a ponderable quantity of coal, or other refiduum; but the real caufe of the increafe of weight, appears to be the air, which can fcarcely be completely excluded, and which, after the wax is burned, combines with the fuperficial part of the oxide, and converts a portion of it into the red or peroxide; fo that the furface in the crucible appears brown, when compared with the interior.

To this caufe, therefore, I am inclined alfo to attribute a fmall part of the increafe obferved in the weight of the iron obtained by the preceding analyfes.

§ V.

Comparative analyfis of other pyrites, by acidifying the whole of the fulphur.

Before I make any obfervations on the nature of the fulphuret which has been proved to conftitute the magnetical pyrites,

* Syftem of Chemiftry, 2d edition, Vol. I. p. 147.

pyrites; it may be proper to ſtate ſome comparative analyſes which I have made of ſeveral of the common pyrites; and, as the method employed was preciſely the ſame as that which has been deſcribed, all that ſeems to be requiſite, is to give an account of the reſults.

In each analyſis, the whole of the ſulphur was converted into ſulphuric acid, which was precipitated by barytes; and, in the ſelection of the ſpecimens, great attention was paid to take the internal parts of the fragments, and not to make uſe of any which exhibited an appearance of decompoſition, or of extraneous ſubſtances.

The iron was, as before, reduced to the ſtate of black oxide; and the addition of weight in each ſeparate analyſis, correſponded, within a few fractional parts, with the proportion of oxigen requiſite to form into black oxide a given quantity of metallic iron, equal to that which in each pyrites was aſcertained to be the real proportion, by deducting the quantity of ſulphur from the total quantity of each pyrites.

The iron, therefore, in theſe is completely metallic, and as ſuch is ſtated in the following reſults:

The component parts of the common pyrites were, at a medium, 9 ſulphur and 8 iron, with no great variation.

No. 1. Pyrites in the form of dodecaedrons with pentagonal faces. Specific gravity 4830.	Sulphur	52.15
	Iron	47.85
		100.
No. 2. Pyrites in the form of ſtriated cubes.	Sulphur	52.50
	Iron	47.50
		100.
No. 3. Pyrites in the form of ſmooth poliſhed cubes, found in the lapis ollaris which accompanies the magnetical pyrites. Specific gravity 4831.	Sulphur	52.70
	Iron	47.30
		100.
No. 4. Radiated pyrites. Specific gravity 4698.	Sulphur	53.60
	Iron	46.40
		100.
No. 5. A ſmaller variety of radiated pyrites. Specific gravity 4775.	Sulphur	54.34
	Iron	45.66
		100.

Conſidering the difference in the figure, luſtre, and colour of theſe pyrites, I expected to have found a much greater difference

difference in the proportions of their component ingredients; but, as the results are the average of several experiments, I have not any reason to doubt their accuracy.

The pyrites crystallized in regular figures, such as cubes and dodecaedrons, according to the above analyses, contain less sulphur, and more iron, than the radiated pyrites, and perhaps than others which are not regularly crystallized. This difference, however, is not considerable; for the dodecaedral pyrites, which afforded the smallest quantity of sulphur of any of the regularly crystallized pyrites, yielded 52.15; and the radiated pyrites, No. 5, gave 54.34; the difference, therefore, is only 2.19. So that the mean proportion of sulphur, in all the pyrites which were examined, is 53.24 *per cent.* and, taking the proportion of sulphur in the magnetical pyrites at 36.50 or 37, the difference between this and the mean of the common pyrites will be 16.74 or 16.24. The magnetical pyrites, therefore, is quite distinct, as a sulphuret of iron, from the common martial pyrites; and, in the following observations I shall prove, that a sulphuret consisting of the proportions last mentioned, has till now been unknown as a product of nature.

Regular crystals contain rather more iron than other pyrites.

§ VI.

Although pyrites is one of the most common of mineral substances, yet the discovery of its real nature is comparatively of a late date; for it appears that even Agricola (whose knowledge of mineral bodies was certainly great, considering the state of science in his time) was not acquainted with its characteristic ingredient, namely, *iron*. According to Henckel, this was first noticed by our countryman Martin Lister, a member of this learned Society, who says, "*Pyrites purus putus ferri metallum est.*"

History of pyrites.

From the time of Henckel, pyrites seems little to have attracted the notice of chemists, until Mr. Proust, the learned professor of chemistry at Madrid, published two memoirs, in which he states, that there are two sulphurets of iron, the one being artificial, and the other natural. The first is the sulphuret which is formed in laboratories, by adding sulphur to red-hot iron, or by exposing both of them to heat in a retort. This is distinguished from the second sulphuret, (which is the common martial pyrites,) by its easy solubility in acids, especially in

Experiments of Proust.

Two sulphurets of iron: artificial, which is easily soluble in acids, and natural, less soluble.

in muriatic acid, by the formation of fulphuretted hidrogen gas during the folution of the fulphuret in the laft named acid, by its colour, and by its inferior denfity.

Artificial fulphuret, 6 fulphur and 10 iron; natural, 9 fulphur and 10 iron.

According to Mr. Prouft, the firft or artificial fulphuret is compofed of 60 parts of fulphur, combined with 100 parts of iron; whilft the fecond fulphuret, or common pyrites, confifts of 90 parts of fulphur and 100 of iron.

The excefs of fulphur is eafily feparated from pyrites; but not the other portion.

He moreover obferves, that the fulphur of the firft fulphuret is difficultly feparated; but that the excefs which is in the fecond fulphuret, or common pyrites, is eafily expelled, and is that portion which is obtained by diftillation, the refiduum being then reduced to the ftate of the firft fulphuret *. 100 parts, therefore, of this fubftance, are compofed of 62.50 of iron and 37.50 of fulphur; and 100 parts of common pyrites are, according to this ftatement, compofed of 52.64 of iron and 47.36 of fulphur.

They are the maxima and minima of the fulphurets.

Thefe proportions Mr. Prouft confiders as the minimum and maximum of the fulphurets of iron. For the latter he allows fome variation; but the compofition of the former he regards as fixed by the invariable law of proportions †; although he obferves, that *it has not as yet been difcovered in the mineral kingdom* ‡.

In fupport of thefe affertions, Mr. Prouft ftates,

Experiments of Prouft on this fubject.

1. That the pyrites found near Soria, when diftilled in a retort heated to rednefs, afforded nearly 20 *per cent.* of fulphur.

2. That the refiduum of the above diftillation had loft the external characters and chemical properties of pyrites, and had affumed thofe of the artificial fulphuret of iron.

* *Journal de Phyfique*, Tome LIII. p. 89, and Tome LIV. p. 89. From pp. 91 and 92 of Tome LIV. it is evident, that the author does not mean to affert, that the firft fulphuret contains 60 *per cent.* of fulphur; but that 100 parts of iron are combined with 60 of fulphur, and form 160 of the fulphuret. In like manner, when 90 of fulphur are united with 100 of iron, a fubftance analogous to common pyrites is formed, which weighs 190 grains or parts.

See alfo our Journal, I. 109, 269, 253, for tranflations of thefe excellent memoirs.

† *Journal de Phyfique*, Tome LIII. p. 90.

‡ " La regne minéral, jufqu'ici, ne nous a point encore préfenté le fer fulphuré au minimum." *Journal de Phyfique*, Tome LIV. p. 93.

3. That

3. That when to this refiduum a quantity of fulphur was added, and the whole was diftilled in a degree of heat not too great, the 20 *per cent.* of fulphur, which had been feparated by the firft diftillation, was, by this, again reftored; and the mafs in the retort thus recovered nearly the original colour, luftre, and chemical properties of the pyrites.

4. That, by adding fulphur to iron filings, or fine iron wire, heated to a low red in a retort, a compound is obtained, in which the proportion of fulphur amounts only to about 20 or 30 parts; but, if this compound is again treated with fulphur in a red heat, a fulphuret is formed, which is readily diffolved in acids, and plentifully affords fulphuretted hydrogen gas.

This is the real minimum of the fulphurets of iron, fixed by the invariable law of proportions, (according to Mr. Prouft,) at 59 or 60 of fulphur and 100 of iron, the former being (as I have already obferved) in the proportion of 37.50 *per cent.*

5, and laftly. That when this fulphuret is again mixed and diftilled with fulphur, (due attention being paid to the degree of heat,) the product is found to have affumed moft of the chemical and external properties of the natural common pyrites, denfity alone being excepted.

The magnetical pyrites is that which Prouft confidered as having been only produced by art.

The application of the above obfervations, to the principal fubject of the prefent paper, is fufficiently obvious; for, when it is confidered, that the magnetical pyrites is fo different from the common pyrites, in colour, hardnefs, folubility in fulphuric acid, and more efpecially in muriatic acid, with the copious production of fulphuretted hydrogen gas; when, by analyfis, it has been found to confift of 36 or 37 of fulphur, combined with about 63 of metallic iron; and, when the artificial fulphuret of iron which has been lately defcribed, is proved to agree with the magnetical pyrites in the nature and proportions of its component ingredients, and in every one of the abovementioned properties; it is evident that the magnetical pyrites is identically the fame with this fulphuret, which hitherto has remained undifcovered in nature, and has only been known as a product of our laboratories. In order however more fully to fatisfy myfelf, I made experiments on the artificial fulphuret, which I formed with fulphur and fine iron wire.

The artificial pyrites agrees with the natural magnetic pyrites.

This ſubſtance agreed, in all the properties which have been noticed, with the magnetical pyrites; and the precipitates obtained by adding pruſſiate of potaſh, and ammonia, to the muriatic and ſulphuric ſolutions, were preciſely ſimilar. The ſpecific gravity was 4390, whilſt (as I have already remarked) that of the magnetical pyrites is 4518.

(To be concluded.)

XI.

Anſwer to a Letter of C. L. *with other Remarks, on the Images formed by convex Lenſes. In a Letter from Mr.* Ezekiel Walker.

To Mr. NICHOLSON.

Dear Sir,

Remarks on the Letters of C. L.

C. L. in his letter in your laſt Number, repreſents my firſt paper as deficient in "philoſophical correctneſs," and wanting "the calm ſpirit of philoſophy." But in attempting to prove my errors, he has proved nothing but his own want of knowledge: and to thoſe who wiſh to ſee an example of C. L.'s "*calm ſpirit of philoſophy,*" I beg leave to recommend the peruſal of his letter in Vol. IX. p. 235, of this Journal.—Theſe ſtrange aſſertions are followed by another ſtill more ſtrange.

Quotation, in which C. L. denies Mr. W.'s facts.

He ſays, "*I will beg leave* to wave any attention to the hiſtory of Mr. Walker's experiments upon which I have animadverted, and *to deny his facts.*"

Thinking that I did not underſtand his meaning correctly, I examined the ſide-note, which runs thus: "C. L. declines any further diſcuſſion reſpecting Mr. Walker's experiments with lenſes, *but denies the facts.*"

Mr. W.'s reply.

As there is no reaſoning with a man who will deny facts, I ſhall decline examining the remainder of C. L.'s letter.

Your other anonymous correſpondent, who has denied the truth of my experiments, will be more eaſily anſwered.

Horizontal moon.

The opinions which have been advanced reſpecting the horizontal luminaries, ſince the days of Ptolomy, who was born about the year 70 of the Chriſtian æra, are ſo numerous, that

that it is ſcarcely poſſible to form a new one. This correſpondent has picked up the theory of Des Cartes, which was confuted more than 100 years ago by Mr. Molyneux. See Philoſ. Tranſ. abr. Vol. I. p. 221; or Dr. Hutton's Mathematical Dictionary, Vol. II. p. 74. I ſhall paſs over this writer's optical errors by juſt obſerving, that he has been doing nothing leſs than denying an eſtabliſhed property in optics.

But before I proceed, it may be convenient to thoſe who are not in poſſeſſion of your two Numbers which contain my former papers on this ſubject, to know what that property in optics is, which has met with ſo much oppoſition.

Farther remarks on the image formed by a lens,

I have proved experimentally, that the image of a candle in the focus of a double convex lens, decreaſes in magnitude as the aperture of the lens is contracted *: And I have alſo advanced, that the image of the ſun or moon, in the focus of the object-glaſs of a teleſcope, decreaſes with its aperture †.

aſſerted to vary with the aperture.

Now it is well known to thoſe who underſtand optics, that this property, the truth of which has been denied ſo poſitively, was diſcovered by Sir Iſaac Newton: and a demonſtration of it may be ſeen in the Optics of Newton, Emerſon, Smith, and Martin, and alſo in the Encyclopædia Britannica, &c.

This poſition aſcribed to Newton,

" The breadth of the leaſt circular ſpace into which object-glaſſes of teleſcopes can collect all ſorts of parallel rays, is about the $27\frac{1}{2}$ part of half the aperture of the glaſs, or 55th part of the whole aperture." (Newton's Optics, 2d Edit. p. 73). Hence it is evident, that if you contract the breadth of the object-glaſs from 10 to 1, the breadth of the circle of light, in its focus, will be contracted in the ſame proportion.

becauſe he notes the ſpace of diffuſion in pencils of rays.

Whence it is deduced, that the image alſo will vary.

In my next paper, this intereſting property of light will be further explained.

I am, Dear Sir,

Your humble ſervant,

EZ. WALKER.

Lynn, March 16, 1805.

* Philoſophical Journal, Vol. IX. p. 164.

† Philoſophical Journal, Vol. X. p. 110.

XII.

Account of an Aeroſtatic Voyage, made by M. GAY-LUSSAC *on the 29th Fructidor, in the Year* 12. *Read to the National Inſtitute, on the 9th Vendemiaire, in the Year* 13.*

Motives of the aſcent.

IN the account of the firſt aeroſtatic voyage which M. Biot and I had the honour to ſubmit to the Inſtitute, we announced that, from the ſurface of the earth to the height of 3977 metres (13024.675 feet), the magnetic power did not experience any ſenſible diminution; and at the ſame time we ſignified a deſire to undertake new aſcents, to aſcertain this important fact at greater heights. We ſoon found that this was alſo the wiſh of many members of the Inſtitute; and, encouraged by the general intereſt which our firſt voyage had excited, we reſolved on making a ſecond very ſhortly; but our aeroſtat not being capable of carrying us together to a greater height than in our firſt aſcent, it was agreed between us that I ſhould go alone. From this moment all our attention was given to the inſtruments which it was requiſite for me to carry; and their conſtruction, which was again entruſted to M. Fortin, as well as an operation which the balloon underwent to give it more levity, retarded my departure till the 29th of laſt Fructidor.

Conſtruction of the inſtruments.

Horizontal needle.

Inſtructed by the experience of our former aſcent, we made ſome changes in our inſtruments; and, in the firſt place, that the oſcillations of the horizontal needle might be leſs affected by the rotation of the balloon, we cauſed a new one to be conſtructed, only 15 centimetres in length. Thus, its oſcillations being more rapid than thoſe of the balloon, it would be eaſier to determine their duration.

Dipping needle.

We made greater modifications in the dipping needle. That we might not be obliged, at each obſervation, to replace it in the magnetic meridian, and to adjuſt its axis truly horizontal, we ſuſpended the metallic chape which ſupports it by a ſilken thread; and, to judge of its inclination, we fixed a portion of a tranſparent circle, graduated in diviſions, to the chape. The whole apparatus was very light, the tenſion on the ſilk thread but trifling, and the needle could reſume its

* From Annales de Chimie, No. 154. Fructidor, An. XIII.

meridian

meridian with facility. M. Coulomb, after having touched the needle, proved it by the method which he propofed in the Memoirs of the Inftitute; and he found that it gave an inclination of 70.5° of the common fcale. In one of its pofitions, which was that in which it was to remain, it varied 31°.

Azimuth compafs.

In our firft afcent, the under furface of the glafs which covered our azimuth-compafs was covered with water, and we were prevented from feeing the fhadow of a horizontal thread, which ferved us for a ftyle. To avoid this inconvenience, it was fufficient to remove the glafs of the compafs; in other refpects, no alteration was made in its firft difpofition. M. Lepine again fupplied us with two fecond-watches, one of which was a ftop-watch: it was with the latter that I made all my obfervations.

Thermometer.

The thermometer I ufed, was the centigrade mercurial thermometer. To guard it from the action of the fun, we placed it within two concentric cylinders of pafteboard covered with gilt paper, one of which was about four centimetres in diameter, and the other fix. Our hygrometers with four hairs, of the conftruction of M. Richer, were difpofed in nearly a fimilar manner. The two glafs balloons in which I was to bring the air, were exhaufted to nearly a millimetre of mercury, and we were fatisfied by leaving them in this ftate for eight days, that they perfectly retained the vacuum. For fear of an accident, we had provided a third balloon of brafs; but very fortunately it was ufelefs.

Hygrometers.

Barometers.

Our two barometers have not a conftant level; and to get the true barometric heights, we formed a table of comparifon, by placing them under the receiver of the pneumatic machine, and meafuring their depreffion from five to five centimetres, by means of a ftandard, the refervoir of which has a conftant level, and which is provided with a very good fcale. Being no longer obliged to obferve more than the upper level to get the true barometric heights, the number of obfervations was diminifhed to one half; which is of great importance when the attention is divided between perfonal fafety and delicate experiments.

The apparatus for meafuring the electricity of the air rendered ufelefs.

Such nearly were the effential inftruments I took with me in my voyage. I was alfo provided with an apparatus for determining the electricity of the air; but a few moments after I quitted the earth, I loft the two metallic wires which were

to attract the electricity at 50 and at 100 metres below me, and I could not make any ufe of it. It will be feen that we had avoided every thing which could influence our needles; and even our anchor, although fufpended 50 metres below the boat, was of wood armed with copper.

This is not the place to mention the precautions which M. Conté had taken, that this new afcent might be free from danger; but it is to be wifhed that he would himfelf publifh what a long and enlightened experience has taught him on this fubject. For ourfelves, we owe him a great tribute of gratitude for his trouble and the intereft he has taken in our voyages; and we are bound to acknowledge, that if they have been fo fortunate, we are indebted for it to his provident cares.

Departure.

All our inftruments being ready, my departure was fixed for the 29th Fructidor: In fact, on that day I afcended from the Confervatory of the Arts and Manufactures, at 9^h 40′, the barometer being at 76.525 centimetres, the hygrometer at 57.5°, and the thermometer at 27.75°. M. Bouvard, who makes meteorological obfervations daily at the Obfervatory of Paris, judged the atmofphere to be greatly charged with vapour, but without clouds. In fact, I had fcarcely rifen 1000 metres when I perceived a light vapour diffufed through all the atmofphere, which rendered the fight of remote objects confufed.

State of the atmofphere on the earth.

First experiment on the ofcillations of the horizontal needle.

Arrived at the height of 3032 metres (9929.8 F.), I began to make the horizontal needle ofcillate, and this time I obtained 20 ofcillations in 83″, while on the earth, and in other refpects in the fame circumftances, it required 84.33″ to make the fame number *. Although my balloon was affected with the rotatory motion we had perceived in our firft experiment, the rapidity of the motion of our needle permitted me to count 20, 30, and even 40 ofcillations.

Inclination of the needle at a height of 12651.365 feet, the fame as on the earth.

At the height of 3863 metres (12651.365 F.), I found the variation of my needle, taking the mean of the amplitude of its ofcillations, was fenfibly 31°, as on the earth. It re-

* Although I have here indicated hundredth parts of a fecond, I am aware that I cannot obferve fuch fmall fractions; but I obtained them by divifions, becaufe on the earth I generally made 30 ofcillations, which required 126.5″.

quired much time and patience to make this obſervation; becauſe, although borne by the maſs of the atmoſphere, I perceived a ſlight wind which continually deranged the compaſs, and after ſeveral unſucceſsful attempts, I was obliged to give up making new ones. Nevertheleſs, I believe the obſervation I have ſtated above is entitled to ſome confidence.

Some time after I wiſhed to obſerve the dipping-needle; but behold what had happened. The drought, favoured by the action of the ſun in a rarefied air, was ſuch, that the compaſs was ſo much warped as to have bent the metallic circle on which the diviſions were traced, and to be itſelf crooked. The motions of the needle therefore had not the ſame freedom; but independently of this diſappointment, I had obſerved that it was very difficult to obſerve the dip of the needle with this apparatus. In ſhort, when I placed the compaſs ſo as to make the ſhadow of the horizontal thread, which ſerved as a ſtyle, coincide with a fixed line, the motion I had given to the compaſs communicated one to the needle, and by the time this was nearly reſtored to reſt, the ſhadow of the ſtyle no longer coincided with the fixed line. It was neceſſary to place the compaſs again in a horizontal poſition, and during the time which this operation required, every thing was again deranged. Unwilling to perſiſt in making obſervations on which I could place no reliance, I abandoned them entirely; and diveſted of every other care, I gave all my attention to the oſcillations of the horizontal needle. I am nevertheleſs convinced, by the knowledge of the defects of our compaſs, that it is poſſible to employ one more ſuitable, which would determine the dip with great preciſion. In trying this experiment, I lowered the other needles ſeparately in linen bags, to 15 metres below the boat.

The obſervations with the azimuth compaſs not to be depended upon.

Cauſes of its defects.

The experiment ſtill practicable.

That the aggregate of all the reſults which I obtained may be more eaſily ſeen, I have collected them into a table which is at the end of this memoir; and they are placed there as they occurred to me, with the correſponding indications of the barometer, thermometer, and hygrometer. The heights have been calculated, according to the formula of M. Laplace, by M. Gouilly, engineer of bridges and highways, who has been ſo kind as to take this trouble. The barometer not having varied ſenſibly on the day of my aſcent, from ten o'clock to three, to calculate the different heights at which I made

The aggregate of the reſults formed into a table.

Calculation of the heights by the barometer compared with one on the earth;

made my obfervations, the height of the barometer on the earth has been taken at 76.568 centimetres, which was the cafe at three o'clock; a height which, conformably to the obfervations made by M. Bouvard at the Obfervatory, is greater by 0.43 millimetres than that which had been obferved at the moment of my departure. The heights of the barometer in the atmofphere have been brought to thofe which would have been indicated by a barometer with an uniform level placed in the fame circumftances, and the mean between the obfervations of the two barometers has been taken for each height. The temperature on the earth having alfo varied very little between ten and three o'clock, it has been fuppofed conftant and equal to 30.75° of the centigrade thermometer.

and of the temperature.

The irregular variation of the temperature arifes from the change of place during the obfervation.

Now on looking into the table it will be evident, in the firft place, that the temperature follows an irregular courfe with refpect to correfponding heights; which, no doubt, is occafioned by the obfervations having been made fometimes in afcending, and fometimes in defcending, and the thermometer having followed thefe variations too flowly. But if the degrees of the thermometer alone are confidered, which form with each other a continually decreafing feries, a more regular law will be found. Thus the temperature on the earth being 27.75°, and 8.5° at the height of 3691 metres, if the difference of the heights is divided by that of the temperatures, we fhall at once obtain 191.7 metres (627.8175 feet) of elevation for each degree of the reduction of the temperature. By performing the fame operation for the temperatures 5.25° and 0.5°, as well as for thofe of 0° and −9.5°, we fhall find, in both cafes, 141.6 metres (463.74 feet) of elevation for each degree of the reduction of the temperature: which feems to indicate that near the furface of the earth the heat is governed by a law which decreafes lefs than at an elevation in the atmofphere, and that at length it follows a decreafing arithmetical progreffion. If it be fuppofed that from the furface of the earth, where the thermometer was at 30.75° to the height of 6977 metres (22849.675 feet), where it had fallen to −9.5°, the heat had diminifhed as the elevations increafed, an elevation of 173.3 metres (567.557 feet) will correfpond to each degree of the reduction of the temperature.

Difference of elevation correfponding to a degree of the difference of temperature.

The

The courfe of the hygrometer was very fingular. At the furface of the earth it was only at 57.5°, while at the height of 3032 metres it marked 62°: from this point it was continually falling to the height of 5267 metres, where it indicated no more than 27.5°; and from thence to the height of 6884 metres, it rofe gradually to 34.5°. If, by thefe refults, it is wifhed to determine the law of the quantity of water diffolved in the air at different elevations, it is clear that attention muft be paid to the temperature; and on joining this confideration with it, it will be feen that it follows an extremely decreafing progreffion.

Unfteady courfe of the hygrometer.

If now the magnetic ofcillations are confidered, it will be obferved that the time for ten ofcillations, made at different heights, is fometimes above and fometimes below 42.16″, which they require upon the earth. By taking the mean of all the ofcillations made in the atmofphere, ten ofcillations would require 42.20″, a quantity which differs very little from the preceding; but on confidering only the laft obfervations which were made at the greateft heights, the time for ten ofcillations would be a little lefs than 42.16°, which would indicate, on the contrary, that the magnetic power had been a little augmented. Without meaning to draw any confequence from this apparent flight increafe, which may very probably arife from the errors to which experiments of this defcription are liable, I conclude that the total of the refults I now offer, confirms and extends the fact which M. Biot and myfelf had obferved, and which, as well as the univerfal gravitation, proves that the magnetic power does not experience fenfible variations at the greateft heights to which we can afcend.

Magnetic ofcillations at different heights.

The magnetic power is not fenfibly altered at the greateft elevations.

The confequence we have drawn from our experiments, will appear a little too precipitate to thofe who recollect that we were unable to make experiments on the inclination of the touched needle. But if it be obferved that the power which caufes an horizontal needle to ofcillate, is neceffarily dependent upon the intenfity and direction of the magnetic power itfelf, and that it is reprefented by the co-fine of the angle of inclination of this latter power, they cannot avoid concluding with us, that, fince the horizontal power has not varied, neither can the magnetic power have varied, unlefs they fuppofe that the magnetic power can have varied precifely

Probable objections to this conclufion refuted.

ciſely in oppoſition to, and in the ſame proportion as the coſine of its inclination, which is by no means probable. In ſupport of our concluſion we have, beſides, the experiment on the inclination made at the height of 3863 metres (12651.325 feet), which proves that, at that elevation, the variation did not vary in a ſenſible manner.

Further proofs of the magnetiſm being the ſame as on the earth.

When at the height of 4511 metres, I preſented to a ſmall touched needle, and in the direction of the magnetic power, the lower extremity of a key; the needle was attracted, and afterwards repelled by the other extremity of the key, which I brought down parallel to itſelf. The ſame experiment, repeated at 6107 metres, had the ſame iſſue: another very evident proof of the action of terreſtrial magnetiſm.

Heights at which the air was taken for examination.

At the height of 6561 metres (21487.275 feet), I opened one of my two balloons of glaſs; and at 6636 metres (21733.9 feet), I opened the ſecond: the air entered both with a hiſſing noiſe. Finally, at $3^{h}\ 11'$, the aeroſtat being perfectly full, and having only 15 kilogrammes of ballaſt, I determined to deſcend. The thermometer was then at 9.5° below the temperature of melting ice, and the barometer at 32.88 centimetres; which gives for my greateſt elevation above Paris, 6977.37 metres (22850.887 feet), or 7016 metres (22977.4 feet) above the level of the ſea.

Greateſt elevation.

Effects on the animal economy at this height.

Although well clothed I began to feel the cold, particularly in my hands, which I was obliged to keep expoſed to the air. My reſpiration was ſenſibly obſtructed; but I was far from feeling any inconvenience ſufficient to induce me to deſcend. My pulſe and my reſpiration were greatly accelerated; hence from reſpiring very frequently in a very dry air, I could not be ſurpriſed at my throat being ſo dry, that it was with difficulty I ſwallowed bread. Before ſetting out I had a ſlight head-ach, ariſing from the fatigues of the preceding day and the want of reſt in the night, and I retained it all the day, without perceiving that it was increaſed. Theſe are all the inconveniences I experienced.

Clouds at a very great height during this aſcent.

Their extreme height in the former one.

A phenomenon which ſtruck me at this great height, was the ſeeing clouds above me, and at a diſtance which appeared to me to be very conſiderable. During our firſt aſcent the clouds did not riſe above 1169 metres (3828.475 feet), and above, the ſky was in its greateſt purity. Its colour at the zenith was ſo intenſe, that it might be compared with that of Pruſſian

Pruffian blue; but in the laft voyage which I made I faw no clouds below me; the fky was very vaporous, and its colour generally dull. It is perhaps not ufelefs to obferve, that on the day of our firft afcent the wind was north-north-weft, and on the laft, fouth-eaft.

Direction of the wind.

As foon as I perceived that I began to defcend, I thought only of moderating the defcent of the balloon, and of rendering it extremely flow. At $3^h\ 45'$ my anchor touched the ground and held, which gives 34' for the time of my defcent. The inhabitants of a fmall hamlet in the vicinity foon ran to the fpot, and while fome brought the balloon towards them by drawing the rope of the anchor, others, placed under the boat, waited impatiently till they could reach it with their hands, to catch and depofit it upon the earth. My defcent was therefore effected without the flighteft fhock or the leaft accident, and I do not believe that a more fortunate one is poffible. The fmall hamlet befide which I defcended, is called Saint-Gourgon; it is fituated fix leagues north-weft from Rouen.

Defcent.

On my arrival at Paris, my firft care was to analyze the air I had brought. All the experiments were made at the Polytechnic School, under the eyes of MM. Thenard and Greffet, and I trufted as much to their judgment as to my own. We noticed the divifions of the eudiometer alternately, and without communication, and it was only when we were perfectly agreed that we wrote them down. The balloon which was filled with air at 6636.5 metres (21735.537 feet), was opened under water, and we all judged that it had at leaft filled the half of its capacity; which proves that the balloon had retained the vacuum very well, and that no foreign air had entered it. We purpofed to have weighed the quantity of water entered into the balloon, to compare it with its capacity; but not having the neceffary inftruments at hand, and our impatience to know the nature of the air it contained being very great, we did not make that experiment. At firft we ufed Volta's eudiometer, and we analyzed it comparatively with atmofpheric air taken in the middle of the courtyard of the Polytechnic School. The following is the comparative analyfis of thefe two airs.

Analyfis of the air taken at great elevations.

	Analysis of the atmospheric Air.		*Analysis of the Air taken at an Elevation of 6636 Metres.*	
		Measures.		Measures.
First Experiment.	Atm. Air,	3.	Air, - -	3.
	Hyd. Gas,	2.	Hyd. Gas, -	2.
	Residue after the combustion, -	3.04	Residue, -	3.05
		Measures.		Measures.
2d Experiment.	Atm. Air,	3.	Air, - -	3.
	Hyd. Gas,	2.	Hyd. Gas, -	2.
	Residue,	3.05	Residue, -	3.04

Atmospheric air and air taken at a great elevation, are identically the same.

At the same time one measure of very pure oxigen gas required 2.04 measures of hidrogen gas; and this result differing only .01 from that obtained in experiments made on a very large scale, and with great care, in the composition of water, it is evident that great confidence may be placed in our results. They prove therefore that atmospheric air, and air taken at an elevation of 6636.5 metres, are identically the same, and that each of them contains .2149 of oxigen. On analyzing the latter air by hydro-sulphuret of potash, we found in it .2163 of oxigen. I cannot give the result of the comparative experiment made on atmospheric air, because we were unable to collect it; but the proportion of oxigen which I have indicated, is also a little greater than that given by the combustion of hidrogen gas, and is comprized within the limits of the variations found in the composition of the atmosphere at the surface of the earth, and which do not prevent it from being considered as constant.

This fact proved by other experiments.

The identity of the analyses of the two airs made by hidrogen gas, proves directly, that that which I brought did not contain the latter gas; nevertheless I also satisfied myself of it, by burning with the two airs only a quantity of hidrogen gas less than would have been requisite to absorb all the oxigen gas; for I found that the residues of the combustion of the two airs with the hidrogen gas were exactly the same.

Saussure

Sauſſure junior alſo found, by uſing nitrous gas, that air taken in the Col-du-Geant contained, within one hundredth part, as much oxigen as that of the plain; and his father aſcertained the preſence of carbonic acid on the ſummit of Mont-Blanc: Moreover, the experiments of MM. Cavendiſh, Macarty, Berthollet, and Davy, have confirmed the identity of the compoſition of the atmoſphere over all the ſurface of the earth. It may therefore be concluded generally, that the conſtitution of the atmoſphere is the ſame from the ſurface of the earth to the greateſt heights to which we can aſcend.

The compoſition of the atmoſphere is every where the ſame.

Theſe are the two principal reſults which I obtained from my laſt voyage. I have aſcertained the fact which had been obſerved by M. Biot and me, on the ſenſible permanence of the intenſity of the magnetic power at a diſtance from the ſurface of the earth; and beſides, I think I have proved that the proportions of oxigen and azote which conſtitute the atmoſphere, do not vary ſenſibly in the moſt extended limits. Many things ſtill remain to be elucidated in the atmoſphere, and we hope the facts we have already collected will ſo far intereſt the Inſtitute as to induce them to deſire us to continue our experiments.

Recapitulation of the reſults.

TABLE

TABLE OF OBSERVATIONS.

Temperature expreffed in degrees of the centigrade thermometer.	Mean of the indications of the two hygrometers.	Mean height of the barometer in the atmofphere brought to that of a barometer with a conftant level.	Correfponding heights in metres above Paris.	Correfponding heights in Englifh Feet.	Number of magnetic ofcillations.	Duration of ofcillations expreffed in feconds.	Ofcillations reduced to the common number 10.	Correfponding times.	OBSERVATIONS.
°	°	Cent.				″		″	
27.75	57.5	76.525	0.0	0.0	30	126.5	10	42.16	The firft line gives the obfervations which were made on the earth before the departure. All the heights muft be augmented by at leaft 39 metres (20 toifes), if it is wifhed to reckon them from the level of the fea.
12.50	62.0	53.81	3032.01	9929.803	20	83.0	10	41.5	
11.00	50.0	51.43	3412.11	11274.660					
8.50	37.3	49.68	3691.32	12089.073					
10.50	33.0	49.03	3816.79	12499.987	10	42.0	10	42.0	
		45.28	4511.61	14775.522	30	127.5	10	42.5	
12.0	30.9	46.66	4264.65	13966.798	30	125.5	10	41.8	
10.0	29.9	46.26	4327.86	14173.741	20	86.0	10	43.0	
8.25	27.6	44.04	4725.90	15469.322	20	84.5	10	42.2	
6.50	27.5	43.53	4808.74	15746.200	30	128.5	10	42.8	
8.75	29.4	45.28	4511.61	14775.522	30	127.5	10	42.5	
5.25	30.1	42.49	5001.85	16381.058					
4.25	27.5	41.14	5267.73	17251.815	40	169.0	10	42.2	
2.5	32.7	39.85	5519.16	18069.249					
0.5	30.2	39.01	5674.85	18585.133					
1.0	33.0	41.41	5175.06	13498.321	30	126.5	10	42.1	
− 3.0	32.4	37.17	6010.70	19783.292					
− 1.5	32.1	36.96	6107.19	20001.047	20	84.0	10	42.0	
0.0	35.1	39.18	5631.65	18387.343	30	127.5	10	42.5	
− 3.25	33.9	36.70	6143.31	20119.340	20	82.0	10	41.0	
− 7.0	34.5	33.39	6884.14	22545.558	20	83.5	10	41.7	
− 9.5		32.88	6977.37	22850.887					

Experiments

XIII.

Experiments and Obſervations on the Motion of the Sap in Trees. In a Letter from THOMAS ANDREW KNIGHT, *Eſq. to the Right Hon.* SIR JOSEPH BANKS, *Bart. K. B. P. R. S. From the Philoſophical Tranſactions for* 1804.

MY DEAR SIR,

IN the Obſervations on the Deſcent of the Sap in Trees, which I laſt year took the liberty to requeſt you to lay before the Royal Society, I offered a conjecture, that the veſſels of the bark, which paſs from the leaves to the extremities of the roots, were, in their organization, better calculated to carry the fluids they contain towards the roots than in the oppoſite direction. I had not, however, at that time, any experiment directly to ſupport this ſuppoſition; but I thought the forms generally aſſumed by trees in their growth, evinced the compound and contending actions of gravitation, and of an intrinſic power in the veſſels of the bark, to give motion to the fluid paſſing through them. In the account of the experiments which I have now the honour to addreſs to you, I truſt I ſhall be able to adduce ſome intereſting facts, in ſupport of that inference.

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Having ſelected, in the ſpring of 1802, four ſtrong ſhoots of the vine, growing along the horizontal trellis of my vinery, I depreſſed a part of each ſhoot, whilſt it was ſoft and ſucculent, about three inches deep into the mould of a pot placed beneath it for that purpoſe; but without making any wound, or inciſion, in the young ſhoots thus employed as layers.

In this poſition they remained during the ſucceeding ſummer; and, in the autumn, had nearly filled the pots, which were ten inches in diameter, with their roots. As ſoon as the leaves had fallen, the layers were diſengaged from the parent ſtocks; and about five inches of wood, containing one bud, were left, both at the proper and the inverted end of each layer. Every bud was alſo, by previous management, made to ſtand at an equal diſtance from the mould in the pots, and with an equal elevation, of about thirty-ſix degrees. About one inch of wood was likewiſe left at each end of every layer beyond the buds.

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In the fucceeding fpring, the buds vegetated ftrongly, both at the proper and at the inverted ends of the layers, as the experiments of Hales and du Hamel had given me reafon to expect; and, in one inftance, the bud at the inverted end of the layer grew with greater vigour than that at its proper end: but the growth of thefe buds was not the object which I had in view.

I have already ftated, that nearly an inch of wood was left at each end of every layer, beyond the bud; and, to this wood, at the inverted ends of the layers, my attention was chiefly directed: for, if the veffels of the bark poffeffed the powers I attributed to them, I concluded that the fap would be impelled to the inverted ends of the layers, and be there employed in the production of new wood, and roots; and, in this, my expectations were not difappointed. At the proper end of the layers, the wood immediately beyond the buds became dry and lifelefs, early in the fucceeding fummer; the ftems alfo, between the buds and the mould in the pots, increafed in fize as ufual; and nothing peculiar occurred. But, at the inverted end, appearances were extremely different: new wood here accumulated rapidly beyond the buds, and numerous roots, of confiderable length, were emitted, whilft no fenfible growth took place between the bafe of the young fhoots and the mould in the pots.

It having been proved by Du Hamel, that inverted parts of trees readily emit roots, I expected to derive further information from cuttings of this kind: I therefore planted, in the autumn of 1802, forty cuttings of the goofeberry-tree, and an equal number of the common currant-tree; one half of each being inverted. Of the former, not one of the inverted cuttings fucceeded; whereas few of the latter failed; and in thefe I had an opportunity of obferving the fame accumulation of wood above the bafes of the annual fhoots, and the fame mode of growth, in every refpect, as in the inverted vines; except that no roots were emitted at their upper ends. The fame thing occurred, without any variation, in inverted grafts of the apple-tree.

If it be admitted, according to the theory I have on a former occafion laid before you, that the fap defcends from the leaves through the veffels of the bark, and that fuch veffels are, in their organization, better calculated to carry their contents towards the original roots than in the oppofite direction, it will be extremely eafy to explain the caufe of the accumulation of wood,

wood, and the emiffion of roots, above, inftead of below, the bafe of the annual fhoots. The veffels of the bark (the *vaiffeaux propres* of Du Hamel) commencing in the leaves, were formerly traced by M. Mariotte, and fubfequently by myfelf, (being ignorant of his difcovery,) to the extremities of the roots; and, when a cutting or tree is planted in its natural pofition, the fap paffes downwards through thefe, to afford matter for new roots, and to increafe the bulk of thofe already formed, having given proper nutriment to the branches and trunk in its defcent. But, in the inverted cutting, or tree, thefe veffels become inverted; and, if their organization be fuch as I have fuppofed it, a confiderable part of that fluid, which naturally defcends, will be carried upwards, and occafion the production of new wood, above, inftead of below, the junction of the annual fhoot with the older wood, as in the experiments I have defcribed. The force of gravitation will, however, ftill be felt; and by its agency, fufficient matter to form new roots may be conveyed to thofe parts of the inverted cutting, or tree, which are beneath the foil. Befides, if we fuppofe a variation to exift in the powers or organization of the veffels which carry the fap towards the root, we may alfo attribute, in a great meafure, to this caufe, the different forms which different fpecies or varieties of trees affume; for, if the fluid in thefe veffels be impelled with much force towards the roots, little matter will probably be depofited in the branches, which, in confequence, will be flender and feeble, as in the vine; and there is not any tree that has been the fubject of my experiments, in which new wood accumulated fo rapidly at the upper end of inverted plants. To an excefs of this power, in the veffels of the bark, we may alfo afcribe the peculiar growth of what are called weeping trees; for, by this power, the effects of gravitation will be, in a great degree, fufpended; and the pendant branch will continue healthy and vigorous, by retaining its due circulation. The perpendicular branch will, however, ftill poffefs fome advantages; for in this, gravitation will act on the fluid defcending from the leaves; and thefe will of courfe abforb from the atmofphere with increafed activity. A greater quantity of matter will therefore enter, within any given portion of time, into veffels of the fame capacity; and this increafed quantity may frequently exceed that which the veffels of the bark are immediately prepared to carry away. Much new wood will in confequence be generated,

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and increaſed vigour given; and, the ſame cauſes operating through ſucceſſive ſeaſons, will give the aſcendancy we generally obſerve in the perpendicular branch.

In the preceding experiments, none of the layers, or cuttings, exceeded a few inches in length; and, to the ſummit of theſe the ſap appeared to riſe, through the inverted tubes of the wood, nearly as well as in thoſe which retained their natural poſition. But ſome former experiments had induced me to ſuſpect, that this would not be the caſe in longer cuttings; I therefore planted in the autumn of 1802, twelve cuttings of the ſwallow, *(Salix caprea,)* inverting one half of them. The whole readily emitted roots, and grew with luxuriance; but their modes of growth were extremely different. In the cuttings which ſtood in their natural poſition, vegetation proceeded with moſt vigour at the points moſt elevated; but in the inverted cuttings, it grew more and more languid as it became diſtant from the ground, and nearly ceaſed, towards the concluſion of the ſummer, at the height of four feet. The new wood alſo, which was generated by theſe inverted cuttings, accumulated above the baſes of the annual ſhoots, as in the preceding inſtances.

Theſe facts appear to prove, that the veſſels of plants are not equally well calculated to carry their contents in oppoſite directions; and, I think, afford ſome grounds to ſuſpect that the veſſels of the bark, like thoſe which conſtitute the venous ſyſtem of animals, (to which they are in many reſpects analogous,) may be provided with valves, whoſe extreme minuteneſs has concealed them from obſervation.

The experiments, and ſtill more the plates, of Hales, have induced naturaliſts to draw concluſions in direct oppoſition to the preceding. But the Plates of that great naturaliſt are not always taken correctly from nature; * and Plates, under ſuch circumſtances, however fair and candid the intentions of an author may be, will too often be found ſomewhat better calculated to ſupport his own hypotheſis, than to elucidate the facts he intends to ſtate.

The preceding peculiarities in the growth of inverted cuttings, appear to have eſcaped the obſervation of Du Hamel; and, as very few inſtances of error, or want of accurate obſervation,

* The eleventh Plate (Vegetable Staticks) is that to which, in this place, I particularly allude.

will

will ever be found in the works of that excellent naturalift, I muft requeft permiffion to fend you fome of the fubjects of my experiments, as vouchers for my own accuracy.

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Of the inverted cuttings employed by Du Hamel, a fmall portion only appears to have remained above the ground; and under fuch circumftances, the different forms of thofe growing in their natural, or inverted, pofition would be fcarcely obfervable. It appears alfo, from his experiments, that fuch inverted cuttings, in fubfequent years, grow with as much vigour as others that are not inverted; whence we muft conclude, that the organization of the internal bark becomes again inverted, and adapted to the pofition of the branch. The growth of fome inverted plants of the goofeberry-tree, which I obtained, many years ago, from layers, gave me reafon to draw a different conclufion; for thefe always continued weak and dwarfifh. I do not, however, entertain the flighteft degree of doubt, but that the affertion of Du Hamel is perfectly correct.

I intended to have added fome obfervations on the reproduction of buds and roots of trees; but thefe would neceffarily extend the prefent Paper to an immoderate length; I fhall therefore referve them for a future communication, and conclude with an account of an experiment which more properly belongs to the Paper I had the honour to addrefs to you laft year, but which had not then fucceeded.

I have ftated, in that Paper, that the leaf-ftalk, the fruit-ftalk, and the tendril, of the vine, had been fuccefsfully fubftituted, in many inftances, for each other; but that I had failed in my efforts to engraft a bunch of grapes, by approach, on the leaf-ftalk; owing, I conceived, to the operation having been improperly performed. In thofe experiments, I cut the leaf-ftalk into the form of a wedge, and made an incifion in the fruit-ftalk, adapted to receive it; but, under fuch circumftances, the leaf-ftalk (as I had proved by many experiments) has no power to generate new matter; and the wounds of the fruit-ftalk heal fo flowly, that I readily anticipated the ill-fuccefs of the operation. In the laft fpring, I pared off fimilar portions of the leaf-ftalk and fruit-ftalk; and, bringing the wounded parts into contact, I fecured them clofely together, by means of a bandage, letting the leaf remain. Under thefe circumftances, an union took place; and the fruit-ftalk being then taken off below the point of junction, and the leaf-ftalk above it, the grapes drew their

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their whole nutriment through the remaining part of the leaf-ftalk. They did not, however, acquire their full fize; and the feeds were fmall, and, I think, incapable of vegetating; but this I attribute to the want of nutriment in quantity rather than in quality; for the union of the veffels of the leaf-ftalk with thofe of the fruit-ftalk was very imperfect. The grapes, which were the purple Frontigniac, poffeffed their mufky flavour, in the fame degree with others growing on the fame plant.

There is another experiment in my laft Paper, which I will alfo notice here, becaufe it appears to lead to fome important conclufions, and had been tried only in a fingle inftance. I have there ftated, that the ftem of a young tree became elliptical, by being confined to move only in the fegment of a large circle. This experiment was fuccefsfully repeated during the laft year, on other trees; but I have nothing to add to the defcription which I have already given.

I am, &c.

T. A. KNIGHT.

XIV.

Extract of a Memoir of Mr. ERMAN, *entitled, Obfervations and Doubts concerning Atmofpheric Electricity*.*

MR. ERMAN who is already fo advantageoufly known to philofophers, has publifhed towards the end of laft year, a memoir which appears to deferve their attention in an uncommon degree.

Signs of atmofpheric electricity differ according to the inftrument.

When he made experiments upon the electricity of the atmofphere, he obferved a great difference between the refult prefented by an electrometer armed with a conductor which was fuddenly raifed from the earth, according to the known method, and that of a metallic rod of much greater length which was infulated and fixed.

Defcription of the electrometer.

The electrometer he ufed was that diftinguifhed in Germany by the name of the electrometer of Weifs. The length of its leaves of gold is half an inch, and the diameter of the glafs tube which enclofes them is three quarters of an

* Journal de Phyfique, Thermidor 12, or Vol. LIX. 98.

inch

inch in diameter, the height being an inch and a half. Its cover of ivory does not project above the glafs, and is perforated in the middle with a hole in which a fmaller glafs tube is fixed, and through this laft tube paffes the metallic rod that ferves to fufpend the gold leaves, and is upon the whole five feet in length, being compofed with feveral pieces fcrewed together, in order that they may be more eafily conveyed from place to place by feparating them. A divergence of three lines in thefe leaves anfwers to two lines in the electrometer of Volta; and that degree of electricity which would produce a divergence of two lines and a half in this laft, caufes the leaves of Mr. Weifs to touch the fides of the glafs cylinder.

Mr. Erman walked in the fields with his electrometer, having its rod confined to the length of three feet. When he fuddenly raifed it from the ground, he obferved a ftrong divergence of *plus* electricity. When he lowered it with the fame fwiftnefs, he perceived an equal great divergence of *minus*. The point of a fixed metallic rod of much greater length, erected at the fame fpot, did not give the leaft indication of electricity.

When raifed fuddenly it diverges *plus*.

An electrometer placed on a poft of three feet in length, produced no divergence; and, when it was afterwards flowly raifed one foot, or a foot and a half, it likewife afforded no fign; but when it was very quickly brought down to the poft, it exhibited *minus* electricity to fuch a degree, that the gold leaf touched the fides of the glafs. This charge was gradually difperfed in the air, or by touching the electrometer with the hand*; but by raifing the inftrument with equal fpeed above the poft, a divergence of *plus* electricity of the fame ftrength was afforded which was deftroyed in the fame manner.

If flowly raifed it does not diverge; but if then fuddenly depreffed it diverges *minus*.

The more infulating the air the lefs it is neceffary to raife the electrometer in order to produce this effect; but the longer the point, the more ftrikingly will the effect be fhewn, even in damp weather. A circular motion at a conftant diftance above the earth does not exhibit any electricity, neither does a progreffive motion upon level ground afford any change;

Conducting power of the air interrupts the refults.

* From this fact it appears, that the electrometer had not any flips of metal within the tube like thofe of Bennet, who firft ufed gold leaves. N.

but

but when the ground riſes even inſenſibly, the phenomenon is obſerved, and it is rather extraordinary, that it even affords a means of aſcertaining the level.

Variations of the experiment.

To vary the experiment, the electrometer may be placed upon a low ſupport, and touched with an inſulated ſtick or wire which has been quickly moved towards the ground, and is afterwards raiſed with the ſame quickneſs. The electrometer in the firſt caſe exhibits *plus* electricity, and in the ſecond *minus*.

It is very remarkable, that the *minus* electricity paſſes through Zero before it changes into *plus*, and *vice verſa*, accordingly as the inſtrument is raiſed or lowered even when there is no interval between the two motions; but the divergence will be more viſible, if time be allowed for the electrometer to diſcharge itſelf between the two experiments.

Sauſſure's obſervations.

Sauſſure formerly obſerved the eſſential requiſites in this phenomenon; but his electrometer was not ſo ſenſible, and he explained the phenomenon by ſuppoſing that the electrometer was the moſt inſulated, and the fixed rod not ſufficiently ſo to indicate ſuch ſlight degrees of electricity.

The electrometer is affected not by any electricity in the atmoſphere, but ſimply by the vicinity of the earth which appears to poſſeſs the *minus* ſtate.

Mr. Erman has aſcertained, that the phenomenon muſt be attributed to the manner in which the electricity is diſtributed in bodies. The maſs of the ground exerts its influence very ſenſibly to a certain diſtance, and the electric atmoſpheres act upon each other without any intervention of the air. The inſulated metallic rod which we raiſe from the ground by holding it perpendicularly, undergoes no other change than is owing to its own proper electricity, which is then leſs compenſated by the influence of the ground. Whether the electrometer or the plate of an electrophorus be ſlowly raiſed, the electricity becomes communicated to the ſurrounding air, and is not manifeſted.

The termination of the conductor or even its being encloſed in glaſs, makes no difference.

A ball placed on the point of the rod of the electrometer does not at all change the divergence of the leaves; and even when the conducting rod of the electrometer is encloſed in a tube of glaſs not open at the top, the phenomena are the ſame, and the divergencies of *plus* and *minus* are equally ſeen according to the direction of the motion. And when a ſecond tube is put over the firſt, for the purpoſe of more exactly preventing the contact of the air, the reſults are not at all changed.

Mr. Erman procured a tube of glafs 14 feet long, which he covered with varnifh, and through which he introduced a metallic wire which paffed above the upper part, and had the electrometer adapted to its lower extremity. A fmall piece of amber being rubbed and applied to the upper end of the wire, produced a divergence in the leaves. The electricity of the air had not, however, any influence on this fixed, and very nicely infulated point; which fhews, that the air furrounding a point is more adapted to deprive it of electricity than to communicate it. Another electrometer which was raifed from the earth at the fame time produced a great divergence.

A fixed rod not affected, becaufe its fituation with regard to the earth is not changed.

Mr. Erman wifhed to know whether two bodies in the fame atmofpheric ftratum reciprocally modify their electric ftate, when their diftance is altered in either direction. Two gold leaf electrometers equally fenfible, and armed with rods three feet long, one of which was bended, fo that its point could be placed near that of the other electrometer, on the fame horizontal line were held at arms length from each other, and touched with the finger to difcharge, and put them in equilibrium with the furrounding air. They were afterwards brought towards each other horizontally, and when they were fufficiently near to permit their atmofpheres to act on each other, there was a divergence of *minus* electricity on both fides, which came to its maximum when the electrometers were in contact. They were again feparated, and the divergence difappeared, both being at the ftate of Zero. One of the wires of thefe electrometers was put in contact with the earth, when the divergence of the other infulated electrometer fuddenly diminifhed to half its former quantity; a fact that alfo proves that the whole depends upon the electricity inherent in bodies, and not at all upon that of the atmofphere.

Two electrometers brought into contact, and then horizontally feparated affect each other according to the laws of bodies actually electrified.

When the electrometers were removed from each other, that which was touched with the finger indicated *plus* in a degree equal to the *minus* it had before fhewn. If the two electrometers be touched with the finger, no further appearance of the *minus* divergence is feen in either of them; but if they be then feparated both will exhibit *plus*. Can thefe phenomena be attributed to the air? or is it poffible to avoid admitting in thefe experiments the influence of electric atmofpheres, and the law of the condenfer?—It is, therefore, well

Continuation of the experiment.

well proved, that all bodies, even which are in equilibrium with the earth, have electric atmospheres in the open air, from which results a modification in their electric state.

The electric influence of a tree was still greater.

Mr. Erman conjectured, that a body of a greater volume might have still more influence. He observed a tree which stood alone in an open space. He retired to the distance of twenty paces from it, and discharged the electricity of his electrometer by touching it with the finger. He then approached the tree, keeping his electrometer at the same height above the earth. The electrometer diverged negatively, and the divergence continually augmented as he came nearer, so that the leaves of gold touched the sides of the glass at the moment when he was under the tree. This negative state lasted as long as he remained there, but when he retired again it gradually disappeared, so as to have become insensible when he arrived at the original distance of twenty feet.

When the communication between the point of the electrometer and the earth was completed under the tree, the divergence ceased; but the electricity was only apparently destroyed, and manifested itself again when he retired from the tree.

The mutual influence of two electrometers is scarcely seen very near the earth. Remarks on aerostatic experiments proposed.

Two electrometers being brought near each other at the distance of half a foot from the earth, did not produce this effect, because the mass of the earth too greatly influenced that proximity. In order to have very simple results, it would be requisite to discover some means of rapidly elevating an insulated body to some thousands of feet above the ground; and if this experiment were undertaken in the boat of a balloon, it would probably be seen, that the positive charge would constantly increase during the ascent, and would become negative as it approached the earth: but if means could be had of changing the system of the balloon in the atmosphere, by the intervention of another body, which would be difficult, the contrary effects might be expected. This change of system is however seen when a cloud descends, and discharges itself with explosion into the earth. But an electrometer of the usual construction in the boat of a balloon would indicate nothing, because the upper part always has a charge of electricity of the same kind as that of the gold-leaves, and of equal intensity.

If

If the influence of the electricity of the ground be sufficient to prevent the divergence, the same effect ought to take place in every close chamber, because a roof or ceiling may be considered as a prolongation of the ground. Hence it is that this polarity does not manifest itself in a chamber; it is also erroneous to pretend that the atmosphere produces electricity; and it is equally erroneous to assert, that electric repulsion does not take place in a vacuum; Mr. Erman proposes to establish in an incontestible manner in another memoir, that the repulsive force of electricity, as well as of magnetism, are also manifested in a vacuum.

These effects cannot take place in a chamber, &c.

If the electricity of the rod of the electrometer could be ascribed to that of the atmosphere, it would follow, that it should be equally electric through its whole length; but if it depend on the earth, the rod will not be equally electrified, and its different parts will be variously modified accordingly as they may be more or less remote from the earth; and this in fact is the case. Two electrometers, each having a pointed rod of three feet, one of which was twice bended in a right angle, so as to have an horizontal portion of three or four inches before it proceeded upwards, were held in such a manner, that the point of the straight rod of the one touched the horizontal part or elbow of the other, which was lower. When they were touched, to discharge them, no divergence was seen, but when they were afterwards separated horizontally, the leaves of the lower electrometer diverged, while those of the upper were not at all moved.

The electric signs do not come from the air; for the rod is not alike electrified throughout.

It is very remarkable that the nearer the contact is made to the upper point of the compound conductor to take its electricity from it, the greater is the positive divergence of the lower electrometer; and if the discharge be made very near the upper point, the electrometer will indicate *plus* but very feebly in comparison with that below.

Its state differs as the contact is made in different parts of its length.

If these two electrometers, the conducting parts of which form together a length of six feet, be raised perpendicularly some feet above the earth, the *plus* electricity will be observed in both; but if they be separated horizontally, the upper one will retain its divergence, and the lower will augment it suddenly, even to double. From this it is evident that the conductor has not the same charge of electricity through all its length, and this is precisely what is called polarity; and as the presence

Electric polarity.

prefence of the electrometer is not effential in this experiment, we may conclude from it that each conductor placed perpendicularly on the earth, has its polarity in the direction in which we have juft explained it.

One extremity of an electrometer indicates divergence; the other does not.

This fhows why an electrometer indicates a divergence *plus* when the point is touched with the hand in the open air, and is foon withdrawn, while there is no divergence when the lower part of the wire is touched. This phenomenon is feen in Weifs's electrometer, even when the wire is not more than three quarters of a foot long.

Experiments on the electricity of the atmofphere uncertain.

From all this it will be feen how deceitful the experiments on the electricity of the atmofphere may be; for in the common method, the hand is directed to be moved from the bottom upwards, towards the point of the electrometer, to deprive it of its accidental electricity, and it is precifely this which communicates it to it.

The intervention of a cloud, rain, &c. reverfes the phenomena;

With refpect to the exceptions which the facts announced above may be liable to, it muft be obferved that when a ftorm drives a cloud over the zenith of the obferver, or when rain, hail or fnow falls at the place of obfervation, the phenomena which occur are totally oppofite; the electrometer gives *plus* when it is brought towards the earth, and *minus* when it is removed from it, &c. &c. But this anomaly is only a tranfient effect, and the pretended negative ftate of the atmofphere does not exift, either when the rain is of long duration, or when the fky is entirely covered with clouds; for in the latter cafe the electrometer is pofitive on being raifed, as when the fky is clear. The perturbations fpoken of are only momentary and change continually. It is evident that thefe changes cannot be attributed to the charge of electricity fet at liberty in the air: it is more probable that thefe maffes of clouds modify the electrometer by their atmofpheres, like the tree in the preceding experiment. The perpendicular conductor may be compared to an iron bar, the polarity of which is reverfed accordingly as the fame pole of a magnet is prefented to one or other of its extremities; thefe effects of the meteoric maffes may alfo be imitated by conductors; the fole motion of the hand above the point of an electrometer is fufficient to produce the negative ftate in queftion, and infulation renders thefe effects more fenfible.

but a continued rain or a fky wholly obfcured by clouds does not.

Thefe changes occafioned by the atmofphere of the cloud,

and may be imitated.

(To be concluded in our next.)

SCIENTIFIC

SCIENTIFIC NEWS.

PROFESSOR DANZEL of Hamburgh made, in the beginning of last year, a second experiment of the mechanism he has invented for the direction of aerostatic machines. He is said to have obtained a progress, in a right line, of twelve feet in a second, which is nearly equal to three leagues in an hour, although he did not bring more than half his means into action. On the same day he made an essay with another machine for the same use, the result of which was not less successful. He has since published the principles of his discovery in a pamphlet, entitled, "Basis of the Mechanism for the Direction of Aerostatic Globes, by Professor Danzel, of the Society of Emulation of Abbeville, at Hamburgh."

Mechanism for directing aerostatic machines.

The class of physical and mathematical sciences of the National Institute of France, in its sitting of 2d Pluviose, in the year XII. elected the following corresponding members. Messrs. Leblond, engineer, returned from Cayenne; Bernard, engineer and astronomer, at Bagnols; Simons, physician at London; Crell, German chemist; Thunberg, the successor of Linnæus in Sweden, who made the voyage to Japan; Bugge, astronomer to the king of Denmark; Gosse, chemist at Geneva; Proust, chemist at Madrid; Cugnoli, astronomer at Modena; Reboul, to whom we are indebted for the measures made in the Pyrenees; Mendoza, a Spaniard, settled at London, known by his important works on navigation. They were all correspondents of the Academy of Sciences in 1793.

Correspondents elected by the class of physical and mathematical sciences of the French national Institute.

The same class has prolonged the time for receiving answers to the following prize questions, until the 1st Germinal, in the year XIII. "To determine by experiment the different sources of carbon in vegetables." And, "To determine by anatomical and chemical observations and experiments, what are the phenomena of the torpidity which certain animals, such as marmots, dormice, &c. experience during the winter, with respect to the circulation of the blood, respiration and irritability; to enquire what are the causes of this sleep, and why it is peculiar to these animals."

Prize questions.

The value of the prizes is doubled, and consists of two kilogrammes of gold, about 233*l.* sterling each.

The astronomical prize, instituted by M. de Lalande, to be given to the person, in France or elsewhere, the members of the

Prize queſtions. the Inſtitute alone excepted, who ſhall have made the obſervation moſt intereſting, or publiſhed the memoir moſt uſeful to the progreſs of aſtronomy, was in the public ſitting of the 6th Meſſidor, decreed to M. Joſeph Piazzi, Profeſſor Royal of Aſtronomy and Director of the Obſervatory of Palermo, for the work which he has lately publiſhed under the title: *Præcipuarum ſtellarum inerrantium poſitiones mediæ ineunte ſæculo XIX. ex obſervationibus habitis in ſpecula Panormitana. Panormi,* 1803, one volume in folio.

The ſubject of the mathematical prize propoſed by the claſs, in this ſitting, is the following: To give the theory of the perturbations of the planet Pallas, diſcovered by M. Olbers." The works are to be written in French or in Latin; and are to be received to the 1ſt Germinal in the year XIV. The prize is a gold medal weighing a kilogramme.

The academy of ſciences, literature, and the fine arts of Turin has propoſed the following new prizes.

Claſs of Phyſical and Mathematical Sciences.

Firſt Prize. The electric and galvanic fluids offer ſo many points of analogy, and ſo great a number of different effects, that many philoſophers believe them to be identical, and many others make them two diſtinct fluids:

New experiments are required which ſhall decide, in a definitive manner, on their identity or diverſity.

Second Prize. It will be ſeen in the *Connoiſſance des tems,* for the year XII. page 217, that the refractions which are adopted do not ſhow an agreement in the obſervations of the ſummer and winter ſolſtices of the years 7, 8, and 9, to give the ſame obliquity of the ecliptic, as they ſhould do; and it is clear that a difference, ſuch as is found, of eight ſeconds in the reſult of the calculations, not of one obſervation, or of two, but in the total of ſeveral, made on different days of different years, muſt have ſome cauſe:

A ſatisfactory explanation is demanded.

Claſs of Literature and the Fine Arts.

Queſtion propoſed. *To demonſtrate whether the economical ſcience known by the name of* Statiſtics, *is a new ſcience, and what are the advantages ſtates may derive from it.*

The prize for each queſtion is 600 francs; and memoirs are to be received until the 30th Frimaire of the year XIII. They are to be written in Latin, French, or Italian, and

ſent, free of poſtage, to the academy. The prizes will be declared in the laſt public ſitting (Meſſidor) of the ſame year. Prize queſtions.

The ſociety of emulation of Colmar in its ſitting of the 17th Thermidor, propoſed the following ſubject for a prize of 300 francs, to be decreed in the public ſitting of Meſſidor, in the year XIII.

" 1. What are the readieſt and leaſt expenſive means of converting into animal oil, ſuch remains of animals as have been hitherto loſt to the arts and to conſumption?

" 2. To what arts, and to what kinds of fabrication can this product be applied; and what preparations ſhould this ſubſtance undergo before it is brought into commerce?

" 3. Finally, in an extenſive undertaking, the object of which would be to convert the remains of animals into animal oil, what precautions would be neceſſary in the adminiſtrative police, to prevent the public health from being injured?"

The memoirs are to be written in French, German, or Latin, and ſent before the 13th Meſſidor, free of poſtage, to the Prefect of the Department, or to the Secretary of the Society.

Extract of a Letter from Mr. J. DALTON, *Lecturer at the Royal Inſtitution. Dated, Mancheſter, March* 12, 1805.

A VERY remarkable and ſingular appearance of the *aurora borealis* was obſerved at Mancheſter from 10 to 11 in the evening of Saturday the 23rd of February. The ſky was almoſt completely obſcured with dark clouds, eſpecially to the ſouth, with ſome rain; about 60° above the horizon, on the ſouth meridian, there was a ſpace in which the cloud was leſs denſe, where a very ſtriking vacillating flame arreſted the attention of moſt people who happened to be out. It ſometimes ſhone with ſuch vividneſs as to exhibit a ſtream of light right down to the horizon through the thickeſt part of the cloud; and at other times, the whole ſouthern region was illuminated as with a flaſh of lightning. The light ſeemed ſcarcely to reach the zenith, and at breaks in the clouds to the north, no light appeared. Should this phenomenon have been obſerved in the ſouth of England or in France, a compariſon of the obſervations may lead us to form ſome idea of the height of this and other appearances of this meteor, which occur much more rarely of late years than formerly. Remarkable aurora borealis.

To

THE ingenious instrument of I. S. F. *for explosions has not appeared to the Editor to be wanted. Chemical lecturers in this metropolis use a small particle of phosphorus, of the size of a pin's head, and sprinkle upon it a pinch of the oxymuriate of potash, in a Wedgewood's mortar. This small quantity, being struck by a sudden rub of the pestle, gives a loud explosion with the* visible *flame, but the burning matter is too little to endanger the spectators.*

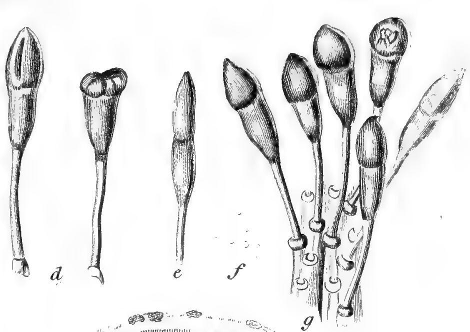
The excellent perspective instrument described and drawn by the Rev. WM. GREGOR, *has no doubt the priority in actual date to that of* R. L. EDGWORTH, *Esq. described in our first Vol. page* 281. *But he will see, by turning to that article, that the instruments are too nearly alike to admit of a second publication.*

I am much obliged to Mr. CUMBERLAND *for his favour of the* 17*th Feb. The useful objects mentioned in his postscript appear to me to be such as would be very acceptable to the public. I am afraid that the circumstance of the invention of Mr.* J. *having been published before would make it less proper to be reprinted in a Journal which is honoured with so much original matter, that presses for insertion. But I shall be happy to give it a more deliberate consideration if his friend will favour me with the loan of the plates, and such additional observations or notes as he may think fit to make.*

The case of deafness, communicated by Mr. HILL *of Wells, Norfolk, in which so much benefit was derived from electricity, is very interesting. Some medical cases have appeared in the early numbers of the first series of this Journal; and others presented themselves for my decision as Editor with regard to their value and importance. But in the attempt to perform this part of my duty, I found difficulties so great, and a subject so extensive, that it appeared proper to confine the papers, to be admitted, to such as are directly and immediately connected with chemical and mechanical science. I shall be happy to convey his letter to a respectable Medical Journal, if he should think fit.*

I am very sorry that the letter from a CORRESPONDENT, *who requested some enquiries to be made, concerning the fundamental experiments on heat, of the late* DR. IRVINE, *has been so long without its effect. I hope to be enabled to give the narrative he requests in the next number.*

Fig.



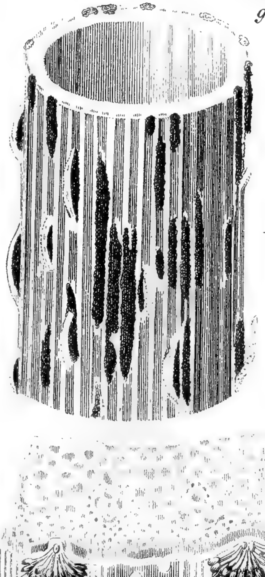


Fig. 5.

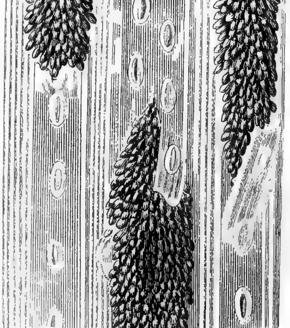


Fig. 6.

Drawn by Francis Bauer.

Engraved by Mutlow Rußell Co.

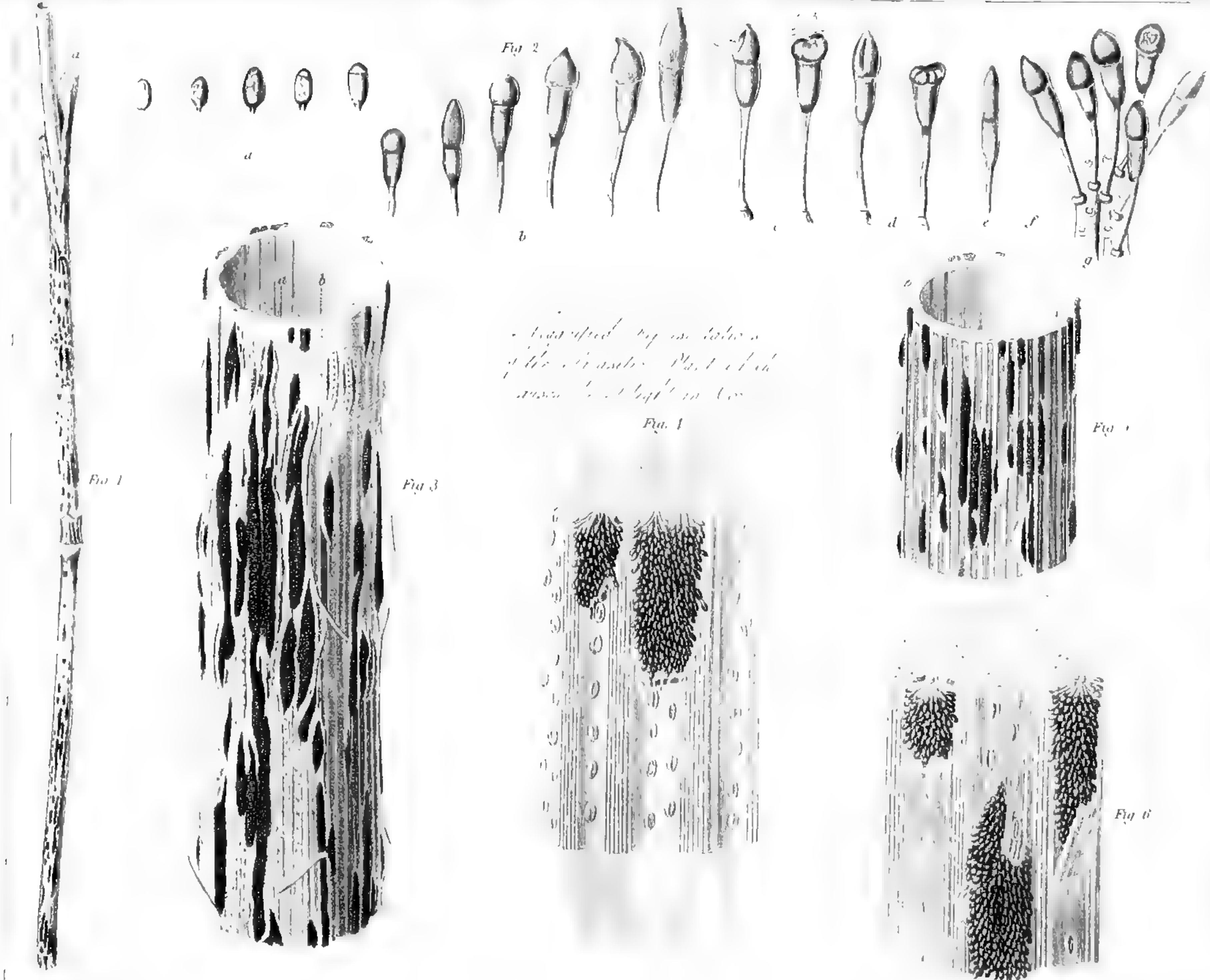


Fig 1
Fig 2
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Fig 3
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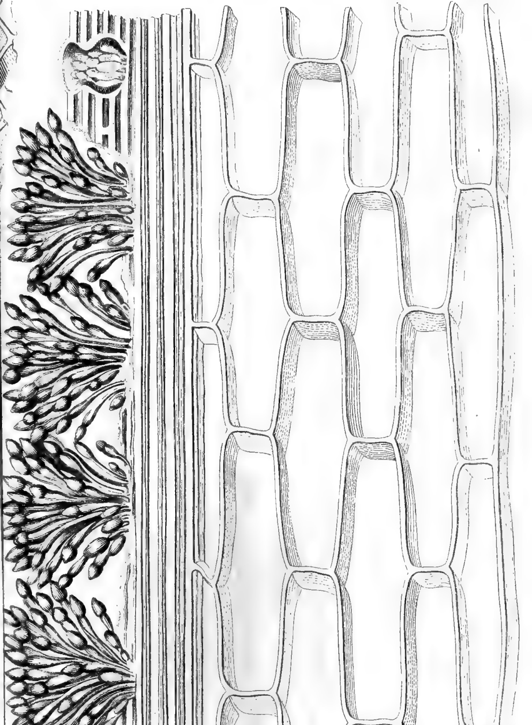
Fig. 2.



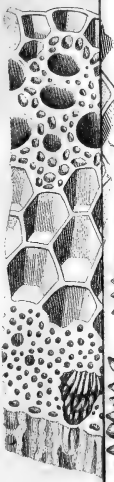


Fig. 3.

Drawn by Francis Bauer.

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Fig. 1

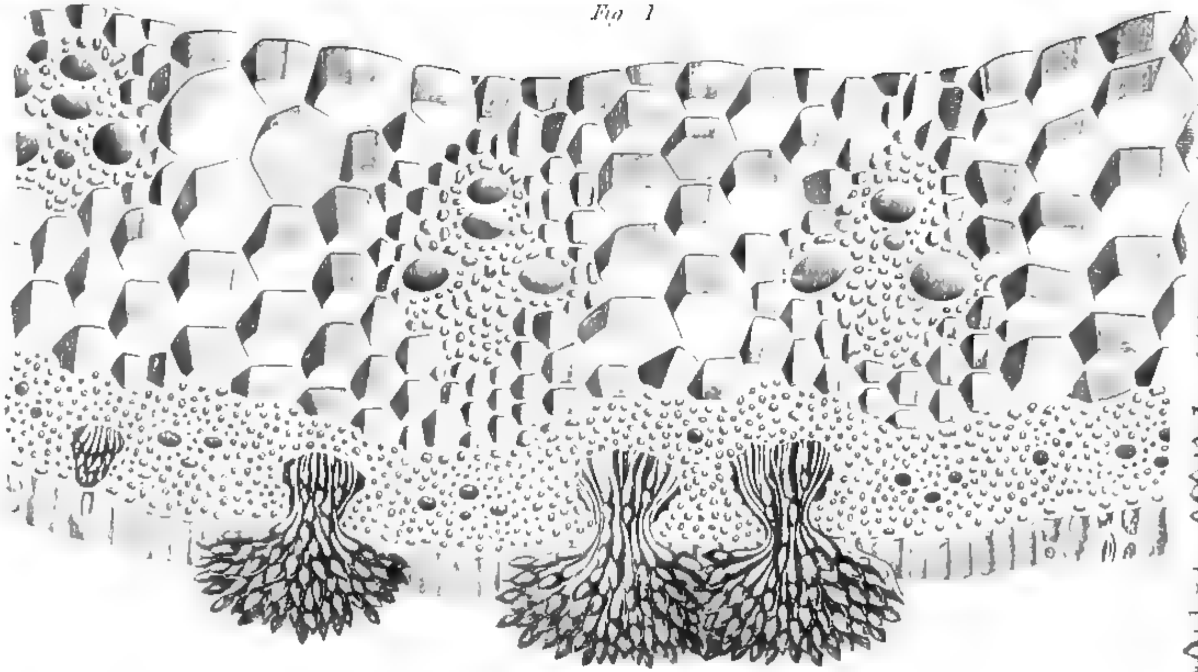


Fig. 2

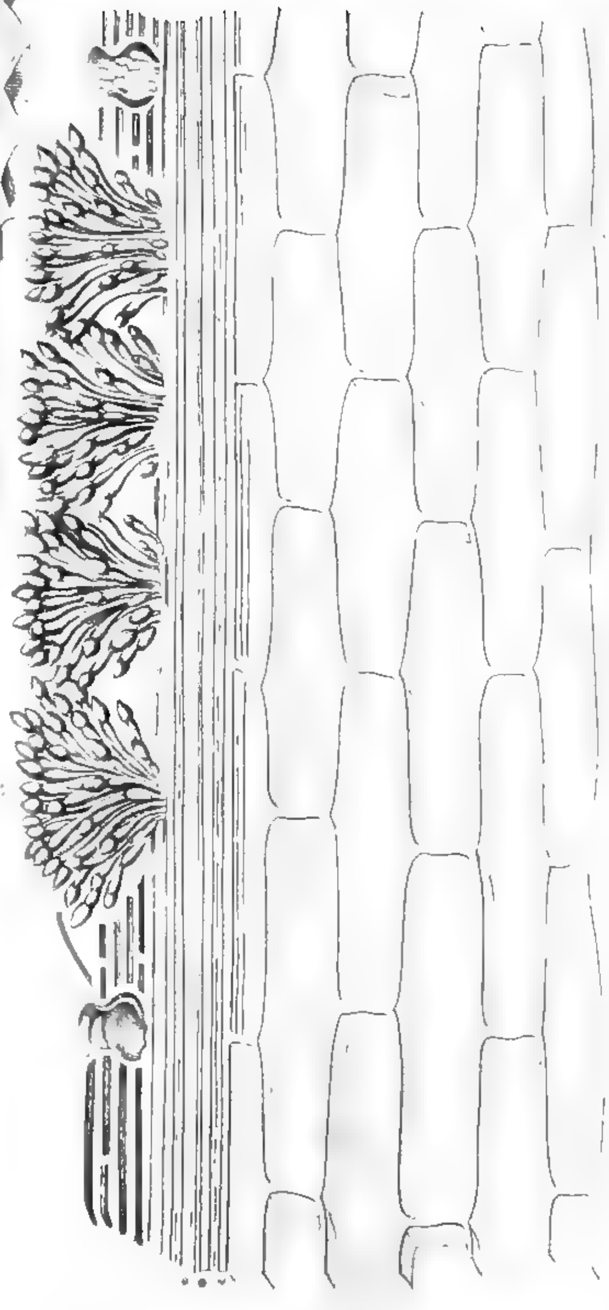
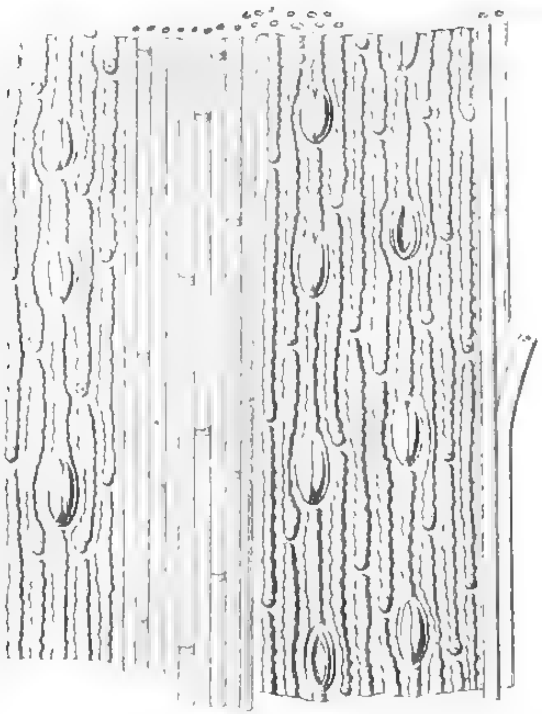
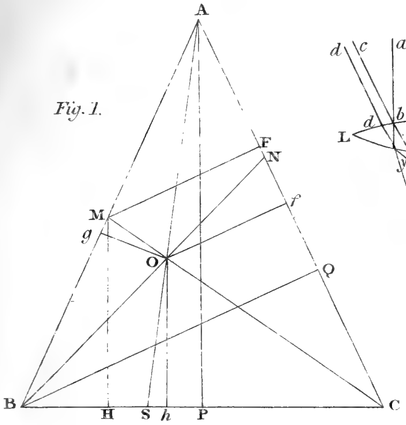


Fig. 3



Highly magnified Sections and Structure of Wheat-Straw, exhibiting the microscopic plant which occasions the Blight in Corn



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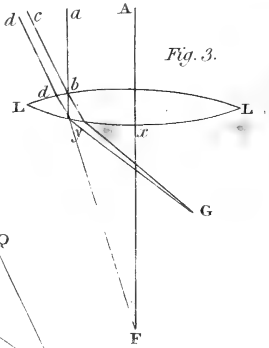

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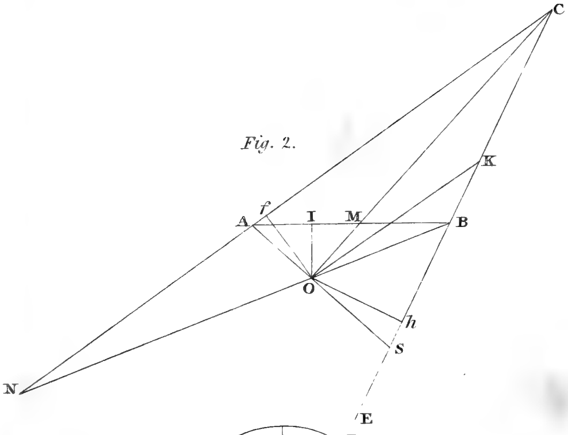

Fig. 2.

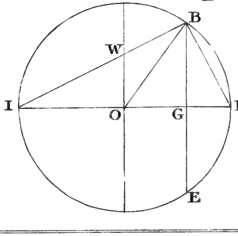

Fig. 4.

Mutlow Sc. Russell Ct.

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W. Stratford, Printer, Crown Court, Temple Bar.